EXHIBIT 12

IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND

MARYLAND SHALL ISSUE, INC., *et al.*,	*	
Plaintiffs,	*	
v.	*	Civil Case No. 16-cv-3311-ELH
LAWRENCE HOGAN, *et al.,*	*	
Defendants.	*	

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SECOND SUPPLEMENTAL DECLARATION OF DANIEL W. WEBSTER

I, Daniel W. Webster, under penalty of perjury, declare and state:

1. I am Bloomberg Professor of American Health in Violence Prevention in the Department of Health Policy and Management at the Johns Hopkins Bloomberg School of Public Health with a joint appointment in the School of Education. I am also Director of the Johns Hopkins Center for Gun Policy and Research and previously served as Co-Director of the Johns Hopkins Center for the Prevention of Youth Violence. I am more than 18 years of age and am competent to testify, upon personal knowledge, to the matters stated below.

2. As part of my prior work on this case, I signed two declarations. My first declaration was signed on August 15, 2018 in support of defendants' motion for summary judgment (ECF 59-19). I also signed a supplemental declaration on November 9, 2018 in further support of defendants' summary judgment motion and in support of defendants' opposition to plaintiffs' cross motion for summary judgment (ECF 89-8).

3. Those prior declarations described, *inter alia*, certain research that I and others have conducted showing that laws that require citizens to obtain a permit to purchase a firearm ("PTP" laws), including Maryland's requirement for a handgun qualification license ("HQL"), promote public safety and reduce firearms violence.

4. Since the time that I signed my prior declarations in this case, my colleagues and I have completed additional research studies that further support my opinions in this case.

5. I co-authored the first of these new studies using new analytic methods and additional data from my prior study of the impact of Missouri's repeal of it permit-to-purchase (PTP) law for handguns on homicides. [1] The new study used annual, state-level data on method-specific homicide rates during 1999-2016 and developed estimates of Missouri's PTP law repeal based on contrasts between changes in Missouri versus other states in the region.[2] When Missouri's changes in firearm homicide rates were contrasted against the set of states in the region that were most similar to Missouri's during the period prior to the law's repeal, we estimated the repeal was associated with a 27 percent increase in firearm homicide rates. The 95 percent confidence interval around that estimate ranged

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[1] Webster DW, Crifasi CK, Vernick JS. Effects of the repeal of Missouri's handgun purchaser licensing law on homicides. *J Urban Health* 2014;91:293-302. Erratum: *J Urban Health* 2014; 91:598-601.

[2] Hasegawa RB, Webster DW, Small DS. Bracketing in the Comparative Interrupted Time-Series Design to Address Concerns about History Interacting with Group: Evaluating Missouri's Handgun Purchaser Law. *Epidemiology* 2019 May;30(3):371-379. doi: 10.1097/EDE.0000000000000989.
PMID: 30969945.

from a 19 percent increase to a 35 percent increase in firearm homicide rates. This article's scientific rigor was recognized by the journal *Epidemiology*—a top journal for epidemiological research—as runner-up for Rothman Prize for best article in 2019. A copy of this peer-reviewed study is attached to this declaration as Exhibit 1.

6. Another study, published in *Criminology & Public Policy,* showed that handgun purchaser licensing laws and bans of large-capacity magazines are associated with significant reductions in the incidence of fatal mass shootings. Daniel W. Webster, *et al, Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States,* 19 Criminology & Public Policy 171–212 (2020). A copy of that article is attached hereto as Exhibit 2. This study used annual, state-level data for 1984-2017 to estimate the association between the adoption and repeal of various state and federal firearm laws on fatal mass shootings after controlling for a variety of demographic, social, economic, and crime variables. In our article published in *Criminology & Public Policy*, we found that PTP laws (also referred to as handgun purchaser licensing laws) were consistently and strongly associated with lower levels of fatal mass shootings after controlling for the presence of other firearm laws and other control variables. In our primary model, we estimate that "handgun purchaser licensing laws requiring either in-person application with law enforcement or fingerprinting (of

applicants) were associated with incidents of fatal mass shootings 56 percent lower than that of other states." [3] (page 181)

7. The most recent study that I co-authored, published this year in the America Journal of Public Health, concluded that State handgun purchaser licensing laws such as the Maryland law at issue in this case—which require a prospective buyer to apply for a license or permit from state or local law enforcement—are highly effective at reducing firearm homicide and suicide rates. Alexander D. McCourt, et al., Purchaser Licensing, Point-of-Sale Background Check Laws, and Firearm Homicide and Suicide in Four States, 1985–2017, 110 Am. J. of Public Health 10, 1546 (October 2020). A copy of this peer-reviewed study is attached to this declaration as Exhibit 3. Using additional data (1985-2017) from prior studies of changes in handgun purchaser licensing laws in Connecticut and Missouri and new statistical models from prior studies, this study shows that handgun purchaser licensing laws are consistently associated with reductions in firearm homicide and firearm suicide rates. Connecticut's handgun purchaser licensing law was associated with a 27.8 percent decrease in firearm homicide rates during the post-law period 1996-2017 and a 32.8 percent decrease in firearm suicides. The estimated effect of the law on firearm suicides was a 23.2 percent decrease through 2006, prior to the beginning of a more robust enforcement of a Connecticut law allowing law enforcement to remove firearms when there was eminent danger, most typically in response to threats of suicide. There were

[3] Webster DW, McCourt AD, Crifasi CK, Booty MD. Evidence Concerning the Regulation of Firearms Design, Sale, and Carrying on Fatal Mass Shootings in the United States. *Criminology & Public Policy*, 2020;19:171–212. doi.org/10.1111/1745-9133.12487

no significant changes in homicide and suicide rates by other methods following Connecticut's 1995 handgun purchaser licensing law. The repeal of Missouri's handgun purchaser licensing law was associated with a 47.3 percent increase in firearm homicide rates and a 23.5 percent increase in firearm suicides. Suicide rates for methods other than firearms did not change in response to the repeal of Missouri's handgun purchase licensing law. There was evidence that nonfirearm homicides increased in Missouri following the law's change; however, the magnitude of the percentage change (+18 percent%) was substantially lower than that of firearm homicide rates. This same study estimated the effects of laws in Maryland (1996) and Pennsylvania (1995) requiring extending background check requirements to private transfers of handguns and found no evidence that background checks without licensing requirements reduced homicide or suicide rates. By contrasting estimates of policy impact of comprehensive background checks for handguns with and without purchaser licensing, this study provided additional compelling evidence that laws requiring handgun purchasers to be licensed have large public safety benefits in preventing homicides and suicides.

8. These studies contribute to a body of research showing that handgun purchaser licensing laws such as Maryland's HQL law are associated with reductions in firearm-related violence and deaths, and provide further support for my opinion that Maryland's HQL licensing requirement promotes public safety and reduces firearms violence.

I hereby declare under penalty of perjury that the foregoing is true and correct.

Date: _October 23, 2020__ _______Daniel W Webster _____________
Daniel W. Webster

Declaration Exhibit 1

Evaluating Missouri's Handgun Purchaser Law

A Bracketing Method for Addressing Concerns About History Interacting with Group

Raiden B. Hasegawa,[a] Daniel W. Webster,[b] and Dylan S. Small[a]

Abstract: In the comparative interrupted time series design (also called the method of difference-in-differences), the change in outcome in a group exposed to treatment in the periods before and after the exposure is compared with the change in outcome in a control group not exposed to treatment in either period. The standard difference-in-difference estimator for a comparative interrupted time series design will be biased for estimating the causal effect of the treatment if there is an interaction between history in the after period and the groups; for example, there is a historical event besides the start of the treatment in the after period that benefits the treated group more than the control group. We present a bracketing method for bounding the effect of an interaction between history and the groups that arises from a time-invariant unmeasured confounder having a different effect in the after period than the before period. The method is applied to a study of the effect of the repeal of Missouri's permit-to-purchase handgun law on its firearm homicide rate. We estimate that the effect of the permit-to-purchase repeal on Missouri's firearm homicide rate is bracketed between 0.9 and 1.3 homicides per 100,000 people, corresponding to a percentage increase of 17% to 27% (95% confidence interval: 0.6, 1.7 or 11%, 35%). A placebo study provides additional support for the hypothesis that the repeal has a causal effect of increasing the rate of state-wide firearm homicides.

Keywords: Bracketing; Causal inference; Comparative interrupted time series; Difference-in-difference; Firearm policy; Gun violence; History-by-group interaction; Permit-to-purchase

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The authors report no conflicts of interest.

SDC Supplemental digital content is available through direct URL citations in the HTML and PDF versions of this article (www.epidem.com).

Data and code availability: The data is provided in Table 1. The code that produced the results can be found in the electronic supplementary materials.

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COMPARATIVE INTERRUPTED TIME SERIES DESIGN AND POTENTIAL BIASES

The interrupted time series design is an observational study design for estimating the causal effect of a treatment on a group when data are available before the group was treated. In the simplest interrupted time series design, the before and after treatment outcomes are compared. This before-after design does not account for confounding factors that co-occur with treatment such as historical events or maturation.[1] To strengthen the before-after design, it is common to add time series data from a control group that never received the treatment over the same period—the comparative interrupted time series design,[1–4] also called the nonequivalent control group design or method of difference-in-differences. The latter name derives from the concept that the simplest comparative interrupted time series analysis is to take the difference between the difference of the after and before outcomes for the treated group and the difference of the after and before outcomes for the control group. This difference-in-differences estimate is an unbiased estimator of the causal effect of treatment if the treatment and control groups would have exhibited parallel trends in the counterfactual absence of treatment[2] (see Figure 1).

The parallel trends assumption can be partially assessed if there is more than one time point in the before period by assessing whether the groups exhibit parallel trends in the before period.[2] However, even if the trends are parallel in the before period, there could be historical events in the after period that affect the two groups differently, i.e., history interacts with group (other reasons that parallel trends could be violated include differences in maturation, instrumentation, or statistical regression between the groups).[5,6] For example, the outcome measures poor health, country A (treated group) enacts a policy reform, country B (control group) does not enact the reform, and a worldwide economic recession occurs after the reform that has a greater impact on people starting out in poorer health. If country B started out with poorer health, then parallel trends would be violated because country B's poor health would have increased more than country A in the after period in the counterfactual absence of the reform because of the worldwide economic recession. This violation of parallel trends would not happen if A and B started with the

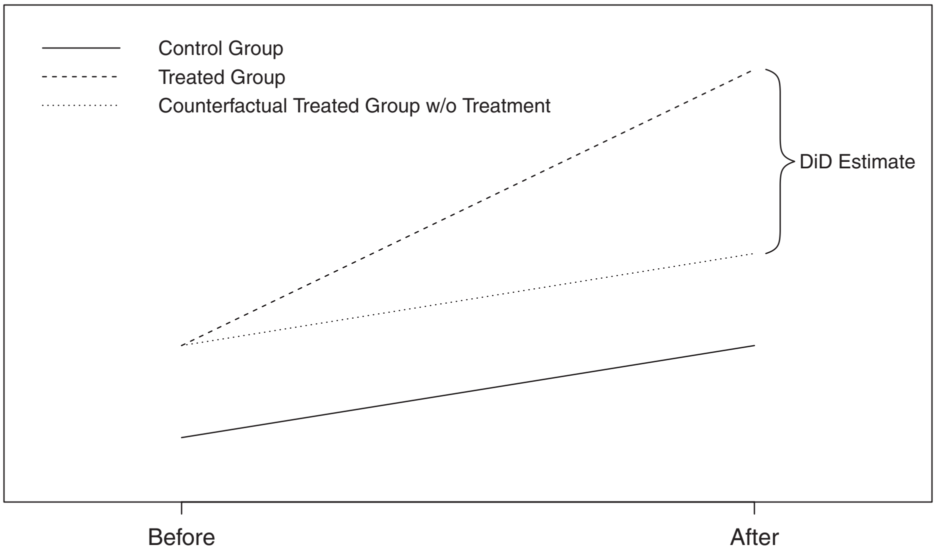

FIGURE 1. Stylized plot of data from a comparative interrupted time series design. The dotted line shows the assumption that the difference-in-difference (DiD) estimate makes about the treatment group's counterfactual mean in the absence of treatment.

same level of poor health in the before period. However, it is often difficult to find a control group that has outcomes close to the treated group in the before period.

When there is no control group completely comparable to the treated group, Campbell[7] proposed bracketing to distinguish treatment effects from plausible biases.[8] Consider the study design of comparing treatment and control at one time point and suppose that there is concern about an unmeasured confounder U. Bracketing uses two control groups such that, in the first group, tends to be higher than in the treated group, and in the second group, U tends to be lower. The effect of U on the treated group is bracketed by its effect on the two control groups. When there is bracketing, if the treated group has a notably higher outcome than both control groups, then this association between treatment and outcome cannot plausibly be explained away as being bias from U.

In this article, we show how bracketing can be applied to the comparative interrupted time series to distinguish treatment effects from plausible biases due to history interacting with group. The basic idea is to consider one control group that has a lower expected outcome than the treated group in the before period and another control group that has a higher expected outcome than the treated group in the before period; we show under certain assumptions that the expectations of the two difference-in-difference estimators using the lower control group and higher control group, respectively, bracket the causal effect of the treatment. Bracketing for the comparative interrupted time series has been mentioned informally,[2] but the idea of choosing the bracketing control groups based on expected before period outcomes was not mentioned. We present assumptions and results for our bracketing method (Methods: Bracketing section) and then apply the method to study the effect of the repeal of Missouri's permit-to-purchase handgun law on its firearm homicide rate (Application: Effect of the Repeal of Missouri's Handgun Purchaser Licensing Law on Firearm Homicides).

METHODS: BRACKETING

Notation and Model

Let Y denote outcome and D dose of exposure, D=1 for treatment and D=0 for control. Let $Y_{ip}^{(d)}$ denote the counterfactual outcome that would have been observed for unit i in period p, p = 0 for before period and p=1 for after period, and had the unit received exposure dose d, i.e., $Y_{ip}^{(1)}$ is the counterfactual outcome under treatment and $Y_{ip}^{(0)}$ is the counterfactual outcome under control. Let $\mathbf{U}_i$ be a vector of time-invariant unmeasured confounders for unit i. Let G denote group where the groups are t = treated group, lc = lower control group (control group with expected outcomes lower than treated group in before period), and uc = upper control group (control group with expected outcomes higher than treated group in before period). Finally, let S be an indicator of whether or not a unit belonging to a particular group is in the study population in a given period. Specifically, S_{ip} = 1 or 0 when unit i is in the population or not in period p: S_{i0}=S_{i1}=1 for a unit in the population both before and after treatment, S_{i0}=1,S_{i1}=0 for a unit in the population only before treatment (unit might have moved away or died in after period), and S_{i0}=0,S_{i1}=1 for a unit in the population only after treatment (unit might have moved into study area or been born in after period).

We consider the following model that generalizes the standard difference-in-difference model and changes-in-changes model.[9] Let $\mathbf{U}_i$ be time-invariant unmeasured confounders and ε_{ip} be an error term that captures additional sources of variation for unit i in period p. Then our model can be expressed as follows:

$$Y_{ip}^{(d)} = h(\mathbf{U}_i, p) + \beta d + \varepsilon_{ip}, \tag{1}$$

where the function $h(\mathbf{U}_i,p)$ is the unobserved expected outcome under control of subject i in period p. We drop the subscript i to refer to a randomly drawn unit from the population

of all units in either period, where $Y_p^{(d)}, d=0,1$, and μ_p are undefined if $S_p = 0$. We make the following assumptions:

$$\text{Increasingness of } h \text{ in } \mathbf{U}: h(\mathbf{U},p) \text{ bounded and increasing in } \mathbf{U} \text{ for } p=0,1. \tag{2}$$

$$((h(\mathbf{U},p) \geq h(\mathbf{U}',p) \text{ whenever all coordinates of } \mathbf{U} \geq \text{all coordinates of } \mathbf{U}')$$

$$\text{Time Invariance of } \mathbf{U} \text{ Within Groups: } \mathbf{U} \text{ conditionally independent of } \{S_0,S_1\} \text{ given group } G. \tag{3}$$

$$\text{Independence of } \varepsilon \text{ with Time and Group: Distributions of } \varepsilon_p \mid S_p=1, G=g \text{ for } p=0,1, \; g=\text{lc, uc, tc all have mean zero and are the same.} \tag{4}$$

Assumptions (2) and (3) match assumptions in the changes-in-changes model. Assumption (2) requires that higher levels of unmeasured confounders correspond to higher levels of outcomes. Such increasingness is natural when the unmeasured confounder is an individual characteristic such as health or ability[9] and Y is a measure of some positive outcome, for example, income. Negative confounders—where higher levels of the confounder correspond to lower levels of the outcome—are not precluded by Assumption (2) as the corresponding coordinates of $\mathbf{U}$ may simply be replaced by their negation. Assumption (3) says that the distribution of confounders in the population of units for a given group remains the same over time. Assumption (4) says that time-varying factors have the same distribution in each group and over time. It would be sufficient for subsequent developments to just assume the distributions of $\varepsilon_p \mid S_p=1, G=g$ for $p=0,1$, $g=\text{lc,uc,tc}$ all have mean zero rather than the stronger assumption of identical distributions (4). We can further relax this assumption by assuming zero mean only for components of ε_p that are true confounders, that is, factors whose distributions depend on the interaction of time and group. Assumption (4) is weaker than the changes-in-changes model assumption that ε_{ip} is always zero which rules out classical measurement error in the outcome when h is nonlinear.[9] Our model contains the standard difference-in-difference model, which can be represented in our model by $h(\mathbf{U},p)=k(\mathbf{U})+\tau p$ for some bounded and increasing function k, where $k(\mathbf{U})$ can be viewed as a group fixed effect.

We make two further assumptions about the distribution of $\mathbf{U}$ in groups and how its effect over time changes among the groups. First, we assume the distribution of $\mathbf{U}$ within groups can be stochastically ordered so that $\mathbf{U}$ is lowest in the lower control group, intermediate in the treated group, and highest in the upper control group:

$$\mathbf{U} \mid G=\text{lc} \preccurlyeq \mathbf{U} \mid G=t \preccurlyeq \mathbf{U} \mid G=\text{uc}, \tag{5}$$

where two random vectors A,B are stochastically ordered, $A \preccurlyeq B$, if $E[f(A)] \leq [f(B)]$ for all bounded increasing functions f.[10] For example, if $\mathbf{U}$ is normally distributed with common variance and group means μ_{lc}, μ_t and μ_{uc}, then $\mu_{lc} \leq \mu_t \leq \mu_{uc}$ would imply (5). Second, we assume that higher values of $\mathbf{U}$ either have a bigger effect over time over the whole range of $\mathbf{U}$ or a smaller effect over the whole range:

$$\text{Either (i) } h(\mathbf{U},1)-h(\mathbf{U},0) \geq h(\mathbf{U}',1)-h(\mathbf{U}',0) \text{ for all } \mathbf{U} \geq \mathbf{U}', \mathbf{U},\mathbf{U}' \in \mathcal{U} \text{ or}$$
$$\text{(ii) } h(\mathbf{U},1)-h(\mathbf{U},0) \leq h(\mathbf{U}',1)-h(\mathbf{U}',0) \text{ for all } \mathbf{U} \geq \mathbf{U}', \mathbf{U},\mathbf{U}' \in \mathcal{U} \tag{6}$$

An example of this pattern of $\mathbf{U}$ confounding could occur in a study of the effect of a regional policy on average income where the policy change occurred contemporaneously with an easing of trade restrictions. A potential unmeasured confounder for such a study would be $\mathbf{U}$ = share of skilled workers in a region, as a higher share of skilled workers is associated with higher average income. There is considerable evidence that trade liberalization leads to an increase in the skill premium—the relative wage of skilled to unskilled workers—at both the regional and country levels.[11,12] Thus, we might expect (i) in (6) to hold if there was an easing of trade restrictions in the after period.

We assume units are randomly sampled from each group in each time period. The data could be obtained from repeated cross-sections or a longitudinal study. Inferences under different sampling assumptions are discussed in eAppendix1; http://links.lww.com/EDE/B503.

Bracketing Result

The standard moment difference-in-difference estimator using control condition c can be written as $\hat{\beta}_{\text{dd}.c} = (\overline{Y}_{1|G=\text{t}} - \overline{Y}_{0|G=\text{t}}) - (\overline{Y}_{1|G=c} - \overline{Y}_{0|G=c})$, where $\overline{Y}_{p|G=g}$ indicates the sample average of units observed in group g and time period p, $Y_p \mid G=g, S_p=1$. This estimate is equivalent to the coefficient on the treatment indicator in a fixed-effects regression with full time and group indicator variables. When using data already aggregated at some level, for example, by state-year, a fixed-effects regression using weights proportional to population will return this estimate. In the following, we show that the expectation of the two standard difference-in-difference estimators computed with the upper and lower controls can be used to bound the treatment effect.

The expected value of the standard difference-in-difference estimator comparing the treated group to the lower control group, $\hat{\beta}_{\text{dd.lc}}$, is

$$\begin{aligned} E[\hat{\beta}_{\text{dd.lc}}] &= \{E[Y_1 \mid G=\text{t}, S_1=1] - E[Y_0 \mid G=\text{t}, S_0=1]\} \\ &\quad - \{E[Y_1 \mid G=\text{lc}, S_1=1] - E[Y_0 \mid G=\text{lc}, S_0=1]\} \\ &= \{\beta + E[h(\mathbf{U},1) \mid G=\text{t}, S_1=1] - E[h(\mathbf{U},0) \mid G=\text{t}, S_0=1]\} \\ &\quad - \{E[h(\mathbf{U},1) \mid G=\text{lc}, S_1=1] - E[h(\mathbf{U},0) \mid G=\text{lc}, S_0=1]\}, \end{aligned}$$

where Y_1, Y_0 denote observed outcomes in after period (p=1) and before period (p=0), respectively. Under the time invariance of $\mathbf{U}$ within groups assumption (3), we have

$$E[\hat{\beta}_{\mathrm{dd.lc}}] = \beta + \{E[h(\mathbf{U},1) - h(\mathbf{U},0) \mid G=t]\} - \{E[h(\mathbf{U},1) - h(\mathbf{U},0) \mid G=\mathrm{lc}]\}; \quad (7)$$

similarly, the expected value of the difference-in-difference estimator comparing the treated group to the upper control group, $\hat{\beta}_{\mathrm{dd.uc}}$, is

$$E[\hat{\beta}_{\mathrm{dd.uc}}] = \beta + \{E[h(\mathbf{U},1) - h(\mathbf{U},0) \mid G=\mathrm{t}]\} - \{E[h(\mathbf{U},1) - h(\mathbf{U},0) \mid G=\mathrm{uc}]\}. \quad (8)$$

The difference-in-difference estimators $\hat{\beta}_{\mathrm{dd.lc}}$ and $\hat{\beta}_{\mathrm{dd.uc}}$ are unbiased if $h(U,1) - h(\mathbf{U},0)$ is constant for all $\mathbf{U}$, or equivalently the effect of the unmeasured confounders is the same in both time periods. If the effect of the unmeasured confounders changes between periods, then because of assumptions (5) and (6), we conclude from (7) and (8) that

$$\min\{E[\hat{\beta}_{\mathrm{dd.lc}}], E[\hat{\beta}_{\mathrm{dd.uc}}]\} \le \beta \le \max\{E[\hat{\beta}_{\mathrm{dd.lc}}], E[\hat{\beta}_{\mathrm{dd.uc}}]\}, \quad (9)$$

i.e., the expected values of the difference-in-difference estimators using the upper control group and lower control group bracket the causal effect (proof in eAppendix2; http://links.lww.com/EDE/B503). The tightness of the bracketing bounds in (9) and, to some extent, the width of the corresponding confidence interval developed in the following section depend on the magnitude of the group-by-time interaction. For example, if urban poverty concentration varied notably between groups and its effect on firearm homicides were modulated by the Great Recession, one would expect looser bracketing bounds.

Inference

We would like to make inferences for the causal effect β under the assumption (6) that $h(\mathbf{U},1) - h(\mathbf{U},0)$ is either an increasing or a decreasing function of $\mathbf{U}$ (we do not want to specify which a priori). Let $\theta_{\mathrm{lc.t}} = E[\hat{\beta}_{\mathrm{dd.lc}}]$ and $\theta_{\mathrm{uc.t}} = E[\hat{\beta}_{\mathrm{dd.uc}}]$, i.e., the expected values of the difference-in-difference estimators using the lower control group and upper control group, respectively. From the bracketing results (9), we have

$$\min(\theta_{\mathrm{lc.t}}, \theta_{\mathrm{uc.t}}) \le \beta \le \max(\theta_{\mathrm{lc.t}}, \theta_{\mathrm{uc.t}}).$$

and the following interval, where CI means confidence interval,

$$\begin{aligned}&[\min(\text{lower endpoint of } 1-\alpha \text{ two-sided CI for } \theta_{\mathrm{lc.t}},\\ &\text{lower endpoint of } 1-\alpha \text{ two-sided CI for } \theta_{\mathrm{uc.t}})\\ &\max(\text{upper endpoint of } 1-\alpha \text{ two-sided CI for } \theta_{\mathrm{lc.t}},\\ &\text{upper endpoint of } 1-\alpha \text{ two-sided CI for } \theta_{\mathrm{uc.t}})],\end{aligned} \quad (10)$$

has probability $\ge 1-\alpha$ of containing both $\min(\theta_{\mathrm{lc.t}}, \theta_{\mathrm{uc.t}})$ and $\max(\theta_{\mathrm{lc.t}}, \theta_{\mathrm{uc.t}})$, and thus β, where it assumed that the two-sided CIs are constructed by taking the intersection of two one-sided $1-(\alpha/2)$ confidence intervals (proof in eAppendix3; http://links.lww.com/EDE/B503).

Constructing the Lower and Upper Control Groups

The results in the previous two sections assume that the lower and upper control groups have been constructed before looking at the data. If the lower control group was constructed by looking at the before period data by choosing units with lower outcomes than the treated in the before period, then the sample average of $Y_0 \mid G=\mathrm{lc}, S_0=1$ may tend to be lower than $E(Y_0 \mid G=\mathrm{lc}, S_0=1)$. Consequently, the difference-in-difference estimate using the lower control group may be downward biased even if the parallel trends assumption holds because of regression to the mean[1]; similarly, the difference-in-difference estimated using the upper control group may be upward biased. This may invalidate the bracketing result (9). To avoid bias resulting from regression to the mean, we propose first selecting a "prestudy" time period prior to the before period. Then, the lower control group can be constructed from units with lower outcomes than the treated in this prestudy period and the upper control group from units with higher outcomes. It should then be tested whether the constructed lower control group has smaller expected outcomes than the constructed upper control group in the before period; see sec:application, for example.

Role of Examining the Groups' Relative Trends in the Before Period

In the standard difference-in-difference analysis that assumes parallel trends, when the before period contains multiple time points, it is a good practice to test for parallel trends in the before period.[2,13] In our bracketing approach, we do not need the parallel trend assumption to hold, but examining the relative trends of the groups in the before period is still useful for assessing model plausibility and assumptions. Our model (1)–(4) along with assumptions (5) and (6) implies that if we had counterfactual data on the treatment group in the after period in the absence of treatment, then, without sampling variance, we would see either (1) the differences between the upper control and counterfactual treated groups and the difference between the counterfactual treated and lower control groups in the after period would be at least as large as their respective differences in the before period or (2) the difference between the upper control and counterfactual treated groups and the difference between the counterfactual treated and lower control groups in the after period would be no larger and possibly smaller than their respective differences in the before period. The following two patterns would violate the model/assumptions: (3) the difference between the upper control and counterfactual treated groups is larger after than before and the difference between the counterfactual treated and lower

control groups is smaller after than before or (4) the difference between the upper control and counterfactual treated groups is smaller after than before and the difference between the counterfactual treated and lower control groups is larger after than before. Although we do not have the counterfactual treatment group's data in the absence of treatment in the after period, we have the treatment group's data in the absence of treatment in the before period. We can split the before period into two (or more) periods and test whether the pattern in the before period is consistent with the model. Visual inspection of the relative trends of the counterfactual treated group and the upper and lower control groups during the before period can provide additional evidence for or against the model assumptions.

Time-Varying Confounders

Our bracketing method addresses an interaction between history and groups that arises because the time-invariant unmeasured confounders that differ between the groups in the before period ($\mathbf{U}$) become more (or less) important in the after period (assumption (6)). When there are time-varying confounders, the bracketing method still works under certain assumptions. Time-varying confounders can be represented in model (1) by letting $\mathbf{U}$ contain all variables that differ in distribution between the groups in the before period, μ_{i0} be the effect of factors that do not differ in distribution between the groups in the before period and μ_{i1} be the effect of the same factors in μ_{i0} in the after period as well as factors not contained in $\mathbf{U}$ that differ in distribution between the groups in the after period (details on time-varying model in eAppendix4; http://links.lww.com/EDE/B503). If this last set of factors is present, then (4) may not hold. However, the bracketing result (9) still holds as long as (i) in (6) holds,

$$E[\varepsilon_{i1} \mid G=\mathrm{uc}] \geq E[\varepsilon_{i1} \mid G=\mathrm{t}] \geq E[\varepsilon_{i1} \mid G=\mathrm{lc}], \tag{11}$$

or when (ii) in (6) holds,

$$E[\varepsilon_{i1} \mid G=\mathrm{uc}] \leq E[\varepsilon_{i1} \mid G=\mathrm{t}] \leq E[\varepsilon_{i1} \mid G=\mathrm{lc}]; \tag{12}$$

eAppendix4; http://links.lww.com/EDE/B503 contains a proof and sufficient conditions for (11) or (12) to hold. One of these sufficient conditions (condition (c) in eAppendix4; http://links.lww.com/EDE/B503) is analogous to (i) in (6) in that effects on the outcome, be they time effects or those due to contemporaneous shocks to confounders, are amplified at larger values of $\mathbf{U}$.

One type of time-varying confounder is a variable that largely stays the same between time periods but may change modestly. For example, in our study of Missouri's repeal of their permit-to-purchase law in sec:application, urban concentration of poverty might be a confounder and $\mathbf{U}$ contain urban concentration of poverty in the before period. Urban concentration of poverty may stay mostly the same over time but change modestly, where the changes are reflected in ε_1. If the effect of urban concentration of poverty on firearm homicides increased in the after period, then the bracketing result would still hold (with respect to the confounding from urban concentration of poverty) as long as the impact of changes in urban concentration of poverty on firearm homicides were at least as great in the upper control group as Missouri and at least as great in Missouri as the lower control group.

Application: Effect of the Repeal of Missouri's Handgun Purchaser Licensing Law on Firearm Homicides

American federal gun law requires background checks and record keeping for gun sales by federally licensed firearm dealers but exempts these regulations for private sales. However, some states have laws requiring all purchasers of handguns from licensed dealers and private sellers to acquire a permit-to-purchase license that verifies the purchaser has passed a background check. Missouri passed a permit-to-purchase law in 1921, requiring handgun purchasers to obtain a license from the local sheriff's office that facilitated the background check, but repealed the law on 28 August 2007 Webster et al.[14] examined the effect of Missouri's repeal on firearm homicide rates (the rate of homicides committed using a firearm). One of their analyses used a comparative interrupted time series design, comparing Missouri to the eight states bordering Missouri using a before period of 1999–2007 and after period of 2008–2010 (the only available postrepeal data at the time of their analysis), finding evidence that the repeal of Missouri's permit-to-purchase law increased firearm homicide rates (see their Table 1). None of the border states introduced new or made changes to the existing permit-to-purchase laws during the study period. Using a fixed-effect

TABLE 1. Age-adjusted Firearm Homicide Rates per 100,000 Persons from Periods 1994–1998 (prestudy Period Used to Construct Lower and Upper Control Groups), 1999–2007 (Before Repeal Period Where Repeal Refers to Repeal of Missouri's Permit-To-Purchase Handgun Licensing Law) and 2008–2016 (After Repeal Period)

	1994–1998	1999–2007	2008–2016
Missouri	6.1	4.7	6.1
Arkansas	7.3	5.1	5.5
Illinois	7.1	5.1	5.2
Iowa	1.2	0.9	1.2
Kansas	4.2	3.0	3.0
Kentucky	4.1	3.3	3.7
Nebraska	2.2	1.8	2.4
Oklahoma	4.8	3.8	4.8
Tennessee	6.9	5.5	5.4
Population-weighted all controls	5.6	4.2	4.4
Population-weighted upper controls	7.1	5.2	5.3
Population-weighted lower controls	3.5	2.7	3.2

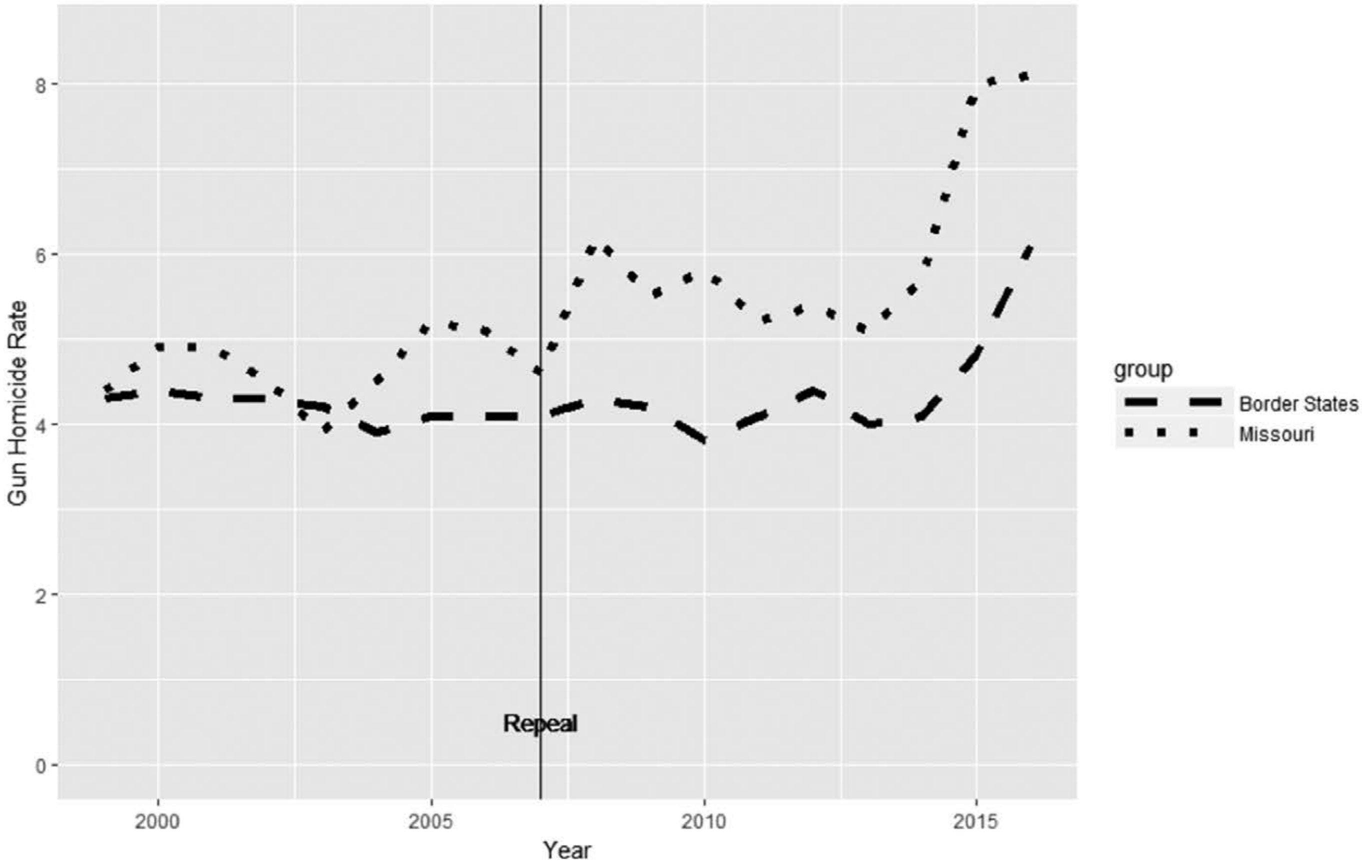

FIGURE 2. Age-adjusted firearm homicide rates in Missouri and states bordering Missouri (population-weighted averages), 1999–2016.

regression and adjusting for several background crime and economic covariates, they estimated that the Missouri permit-to-purchase repeal was associated with an increase in the firearm homicide rate by 1.1 per 100,000 persons (95% confidence interval [CI]: 0.8, 1.4), a 22% (95% CI: 16 %, 29%) increase. Nongun-related homicides remained virtually unchanged. In what follows, we reexamine the effect of Missouri's repeal using bracketing and the now available after period data from 2008 to 2016 to address possible biases arising from unobserved state-by-time interactions. The code and data can be seen in http://links.lww.com/EDE/B501.

Figure 2 shows the age-adjusted firearm homicide rates in Missouri and the border states over the study period using data from the US Centers for Disease Control and Prevention's (CDC) Wide-ranging Online Data for Epidemiologic Research (WONDER) system.[15] The standard difference-in-difference estimate using all neighboring control states, shown in the top row of Table 2, is that Missouri's permit-to-purchase repeal increased firearm homicides by 1.2 per 100,000 persons (95% CI: 1.0,1.4), corresponding to a 24% increase (95% CI: 18%, 31%). In the before period, Missouri had generally higher firearm homicide rates than the control border states, suggesting a lack of comparability between the groups.

TABLE 2. Difference-in-Difference Estimates of Effect of Repeal of Missouri's Permit-To-Purchase Handgun Licensing Requirement on Firearm Homicide Rates per 100,000 Persons

Control Group	Estimate [95% CI]	Corresponding % Change Estimate [95% CI]
All controls	1.2 [0.9, 1.5]	24% [18%, 31%]
Upper controls	1.3 [0.9, 1.7]	27% [19%, 35%]
Lower controls	0.9 [0.6, 1.2]	17% [11%, 23%]

One concern is that the start of the after period coincided with the beginning of the Great Recession. The economic downturn was followed by a decline in homicide rates. Possible reasons for the effect of the downturn on homicide rates and violence generally include changing alcohol affordability, disposable income, unemployment, and income inequality.[16–18] The effects of the economic downturn on firearm homicides might interact with the starting level of firearm homicides in a state. To address this concern, we constructed upper and lower control groups that bracket Missouri's firearm homicide rate in the before period. To avoid regression to the mean (Constructing the Lower and Upper Control Groups), we use data from 1994 to 1998, 5 years before our before period, to choose

the upper and lower control groups; see Table 1 for data. The lower control group is Iowa, Kansas, Kentucky, Nebraska, and Oklahoma, and the upper control group is Arkansas, Illinois, and Tennessee. The population-weighted firearm homicide rate in the before period of 1999–2007 is 5.2 in the upper control states, 4.7 in Missouri, and 2.7 in the lower control states (95% CI for difference between upper control and Missouri: 0.2, 0.8; 95% CI for difference between Missouri and lower controls: 1.8, 2.2).

Figure 3 shows firearm homicides rates (age adjusted and population weighted) in the bracketed control groups compared with Missouri. The bottom two rows of Table 2 show the difference-in-difference estimates using the lower and upper control groups and 95% CIs. Both the lower and upper control groups provide evidence that Missouri's repeal of its permit-to-purchase handgun law increased firearm homicides, bracketing the effect of the repeal between 0.9 and 1.3 homicides per 100,000 people, corresponding to a percentage increase of 17% to 27%. The interval (10) that has a ≥95% chance of containing the effect of the repeal on the firearm homicide rate is [0.6, 1.7], corresponding to an 11% to 35% increase in firearm homicides, providing evidence that the repeal increased firearm homicides.

Assessing Model Assumptions: Time-Varying Confounders and Relative Trends

A type of time-varying confounder that is relevant to the Missouri permit-to-purchase study is a factor that only arises in the after period. The Ferguson unrest in 2014 might have led to less effective policing (spikes in violence typically follow social unrest) in Missouri compared with other states. Such a time-varying confounder would be unlikely to satisfy (11) or (12) because it arises only in the treated group (Missouri) in the after period. However, this confounder alone does not change our finding that the repeal increased firearm homicides. If we limit the study to 2008–2013, Missouri still has larger increases in firearm homicide rates than both the upper and lower control groups; see eAppendix6; http://links.lww.com/EDE/B503.

To assess the plausibility of our models (1)–(4) and assumptions (5) and (6), we apply the relative trends test described in Role of Examing the Groups' Relative Trends in the Before Period. Applying the test to our study of the repeal of Missouri's permit-to-purchase law, we do not find evidence that our model assumptions are violated. Visual inspection of the relative trends of counterfactual Missouri and the upper and lower controls in the before period further supports the plausibility of our model assumptions; see eFigure1 in eAppendix5; http://links.lww.com/EDE/B503.

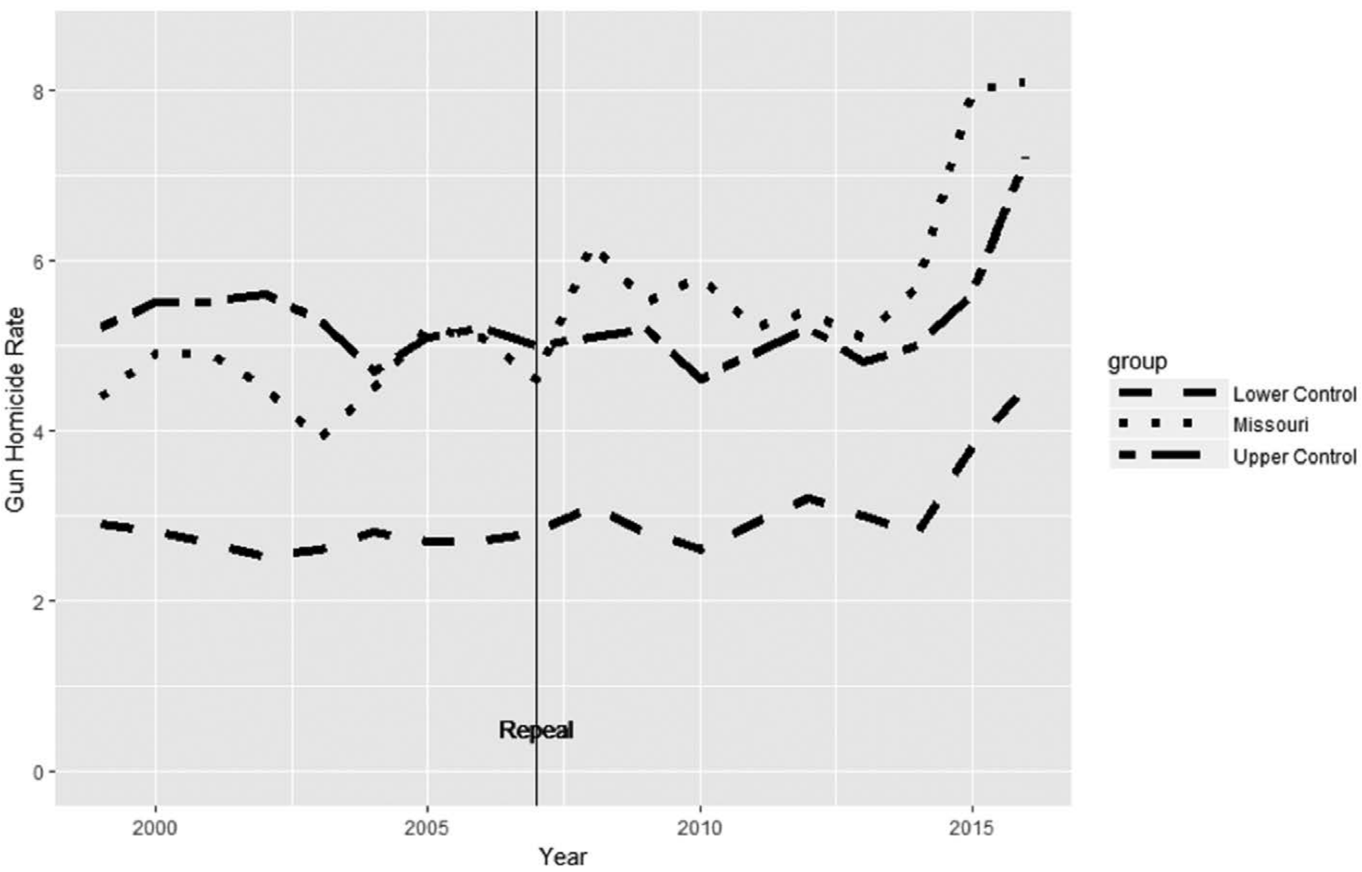

FIGURE 3. Age-adjusted gun homicide rates per 100,000 persons in Missouri, lower control states bordering Missouri (population-weighted averages) and upper control states bordering Missouri, 1999–2016.

STANDARD ERROR ESTIMATES: A POISSON MODEL FOR DEATH COUNTS

The standard errors used for inference in the previous section come directly from the CDC WONDER system. Vital statistics that derive from complete counts of deaths (by cause) are not subject to sampling error. Nonetheless, a stochastic model of vital statistics may be justified by the presence of biological, environmental, sociological, and other natural sources of variability.[19] For inferential purposes, a census may be viewed as a realization from such a stochastic process under similar conditions to those observed.[20] In particular, the observed firearm homicide death rate in any state-year may be viewed as one of a large series of possible Poisson distributed outcomes under similar conditions.[21] The standard errors reported by the CDC are computed under this Poisson model.

A Placebo Study: Assessing Alternative Sources of Uncertainty

There may be other sources of uncertainty unaccounted for by the natural variability of a Poisson model for yearly state-level firearm homicides. Several recent papers suggest that such sources of uncertainty, if ignored, may yield substantially different inferential conclusions. Serially correlated data,[22] yearly state-level shocks,[23] and small numbers of policy changes[24] can cause the standard errors returned by a fixed-effects regression to be downwardly biased. We conduct a placebo study[22,25] to address inferential challenges that arise from the presence of possibly dependent, yearly state-level shocks to the conditions that generate these Poisson realizations.

Akin to permutation inference, a placebo study in the context of the Missouri permit-to-purchase repeal analysis applies the bracketing method to every state to create a placebo intervention effect distribution. Specifically, for each state where there was no permit-to-purchase repeal, we construct lower and upper control groups of neighboring states, when available, in exactly the same way we did so for Missouri. We then compute the difference-in-difference estimates using both control groups for a placebo "repeal" on 28 August 2007. This results in two exact distributions for the placebo intervention effect estimate, one estimated using lower controls and the other using upper controls. If the permit-to-purchase repeal effect in Missouri is not spurious, we would expect to see few placebo effects greater than the ones reported in our study using either control condition. The histograms of the placebo effects in Figure 4 suggest that the Missouri bracketing study is relatively robust to these alternative sources of variability. Of the 38 states that had lower control neighbors, only two (Oklahoma and Delaware) had placebo effect estimates using lower controls that were larger than Missouri (dashed line, Figure 4, left). Of the 37 states that had upper control neighbors, only one (Delaware) had a placebo effect estimate using upper controls that was larger than Missouri (dashed line, Figure 4, right). Alaska, Hawaii, the District of Columbia, and three states with missing data in either the prestudy, before, or after period were excluded from the analysis.

CONCLUSIONS AND DISCUSSION

We developed a bracketing method for comparative interrupted time series to account for concerns that history may interact with groups. In a study of the repeal of Missouri's permit-to-purchase handgun law, the method addressed a concern that on average, control states started out with lower firearm homicide rates than Missouri before the repeal. Comparing both to states that started with higher firearm homicide rates than Missouri and states that started with lower rates, the repeal was associated with a significant increase in firearm

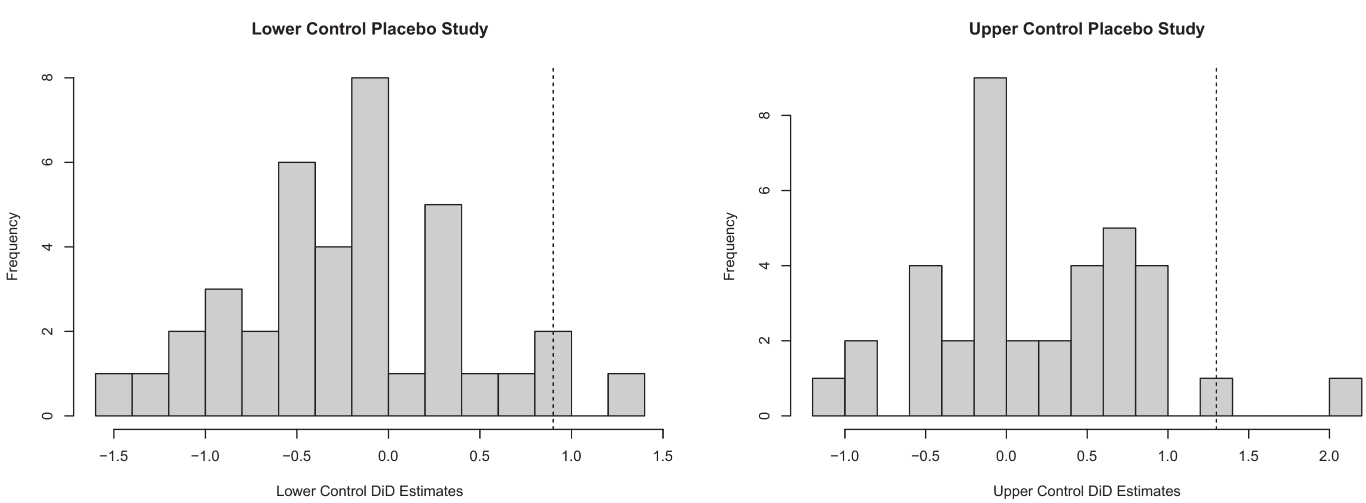

FIGURE 4. Histograms of placebo "repeal" effects using different control states. Left, Histogram of placebo difference-in-difference (DiD) estimates using lower control states (n = 38 states with lower control neighbors, includes Missouri). Two states (Oklahoma and Delaware) had a larger estimate than Missouri (dashed line). Right, Histogram of placebo difference-in-difference estimates using upper control states (n = 37 states with upper control neighbors, includes Missouri). One state (Delaware) had a larger estimate than Missouri (dashed line).

homicides, thus strengthening the evidence that the repeal had a causal effect of increasing firearm homicides.

A limitation of our estimated impact of the repeal of Missouri's permit-to-purchase law is that a Stand Your Ground law was simultaneously adopted in Missouri. However, in the original study by Webster et al,[14] the inclusion of a Stand Your Ground indicator in the regression did not dramatically change the estimated effect. Additionally, a recent comparative interrupted time series study examining firearm homicide rates in large urban counties found that permit-to-purchase laws were associated with significant reductions in firearm homicides after controlling for the effects of Stand Your Ground laws.[26] Further evidence that the contemporaneous Stand Your Ground law does not change the qualitative conclusion of our study can be found in the placebo study. There were 16 additional states that adopted Stand Your Ground laws within a few years of Missouri's permit-to-purchase repeal.[26] Only one state (Oklahoma) of the 16 had a difference-in-difference placebo effect estimate using lower controls that were larger than Missouri, and none of the states had placebo effect estimates using upper controls that were larger than Missouri.

Although only one of many potential patterns of bias, the history-by-group interaction bias addressed in this article has been mentioned in the literature since at least the middle of the 20th century. A version of it is referred to selection-maturation interaction in a taxonomy of possible threats to the validity of experimental and quasi-experimental designs presented in Campbell and Stanley.[27] Fundamentally, bracketing relies on constructing control groups across which this potential source of confounding is systematically varied.[28] Other methods for constructing adequate control groups in the presence of history-by-group interactions, such as the synthetic control method,[25] have also found success in comparative case studies of the effect of permit-to-purchase laws on firearm homicide rates.[29] Although we do not argue that bracketing is uniformly superior to the synthetic control method, the practitioner may find that each has strengths that lend themselves to different settings. When the researcher believes that unmeasured history-by-group confounding, $h(U, p)$, can be expressed as a linear factor model with time-varying slopes and group-specific loadings, the synthetic control method provides an asymptotically unbiased point estimate of the causal effect of treatment while bracketing can only provide bounds on the treatment effect. However, when the practitioner suspects that only the weaker assumptions of the model outlined in Notation and Model. hold, the bracketing bounds will remain unbiased, in that they contain the true effect in expectation, while the point estimate using synthetic controls need not be unbiased; see eAppendix7; http://links.lww.com/EDE/B503 for further discussion. A detailed example of such a case can be found in the eAppendix8; http://links.lww.com/EDE/B503.

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Declaration Exhibit 2

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CRIMINOLOGY
& Public Policy

SPECIAL ISSUE ARTICLE

COUNTERING MASS VIOLENCE IN THE UNITED STATES

Evidence concerning the regulation of firearms design, sale, and carrying on fatal mass shootings in the United States

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Research Summary: We used data from the FBI's Supplemental Homicide Reports and other publicly available databases to calculate state-level annual incidence of fatal mass shootings for 1984–2017. Negative binomial regression models were used to estimate the associations between changes in key gun laws and fatal mass shootings. Handgun purchaser licensing laws and bans of large-capacity magazines (LCMs) were associated with significant reductions in the incidence of fatal mass shootings. Other laws commonly advocated as solutions to mass shootings—comprehensive background checks, assault weapons bans, and de-regulation of civilian concealed carry of firearms—were unrelated to fatal mass shootings.

Policy Implications: Our findings suggest that laws requiring firearm purchasers to be licensed through a background check process supported by fingerprints and laws banning LCMs are the most effective gun policies for reducing fatal mass shootings.

KEYWORDS
mass shooting, gun regulation, EVALUATION

High-profile public mass shootings (e.g., incidents that gain significant media attention as a result of high victim count and/or unique characteristic such as location or motive) prompt what have become predictable responses across the political spectrum. One side points to easy firearm access as the key cause of mass shootings and calls for stronger gun laws including comprehensive background checks, bans on assault weapons and large-capacity magazines (if those were used), and more recently, Extreme Risk Protection Order (ERPO) laws to disarm persons planning violent acts. The other side sees unarmed victims being shot in mass shootings and focuses on the hypothetical question, "What if one of the victims or a bystander used a firearm to stop the attack?" The solutions to mass shootings that stem from this perspective include eliminating so-called "gun free zones" and reducing or eliminating restrictions on civilian carrying of concealed firearms in public places.

In a study of fatal mass shootings in the United States during 2014–2017 with several online data sources, Zeoli and Paruk (2020, issue) determined that 46% of the shootings were committed by someone who was prohibited or likely prohibited from possessing a firearm. But the breadth of disqualifying conditions for firearm possession—e.g., whether convictions for violent misdemeanors, domestic violence restraining orders (DVROs) involving dating partners, and younger than 21 years of age disqualify someone from purchasing or possessing a firearm—vary significantly across states and determine the size of the pool of persons at increased risk for perpetrating firearm violence who are legally prohibited from purchasing or possessing firearms (Vittes, Vernick, & Webster, 2012). Indeed, the breadth of disqualifying conditions for persons with a history of violence was consistently associated with reductions in rates of intimate partner homicides (Zeoli et al., 2018). Because many mass shootings are committed in the context of domestic violence or involve perpetrators with a history of domestic violence (Zeoli & Paruk, 2020), broader firearm restrictions for DVROs and violent misdemeanors could potentially reduce mass shootings.

Broad firearm prohibitions for violent or other criminal actions may not keep those individuals from accessing firearms without strong background check systems. State laws requiring comprehensive background checks (CBCs) and purchaser licensing could also potentially influence firearm availability to individuals at risk of perpetrating a mass shooting by making it harder for prohibited persons to obtain firearms. The typical CBC law requires prospective purchasers in private transfers of firearms to pass a background check that is facilitated through a licensed firearm dealer. In contrast, most purchaser licensing laws require prospective purchasers to apply directly at public safety agencies where they are fingerprinted for thorough background checks that include more complete records of prohibiting incidents and greater time available to conduct those checks than is the case for background checks absent licensing. Some licensing laws also require gun safety training, and a few provide officials the ability to use their discretion to deny an applicant if there is good reason to believe he or she might be dangerous (e.g., some history of violence). Rigorous studies of the impact of state CBC laws have not shown that these laws reduce homicides (Castillo-Carniglia et al., 2018; Kagawa et al., 2018; Zeoli et al., 2018); however, there has been consistent evidence that licensing laws reduce homicides (Crifasi et al., 2018; Hasegawa, Webster, & Small, 2019; Rudolph, Stuart, Vernick, & Webster, 2015) and suicides (Crifasi, Meyers, Vernick, & Webster, 2015). Licensing laws could potentially suppress fatal mass shootings, but there are no rigorous studies examining this question.

The research literature on the effects of firearm policies on mass shootings is sparse and has important limitations. A recent study found that that higher rates of gun ownership and greater permissiveness of gun laws were associated with higher rates of fatal mass shootings for incidents connected to domestic violence and other types of mass shootings (Reeping et al., 2019). Unfortunately, the gun law permissiveness scale used in the study has not been fully described, evaluated, or validated, and it does not allow for estimates of the effects of specific firearm laws on mass shootings.[1] Furthermore, the data to identify fatal mass shootings in this study—the FBI's Supplemental Homicide Reports (SHR)—did

not include major fatal mass shootings, including shootings at Sandy Hook Elementary School in Newtown, Connecticut, in 2012 (26 deaths); a movie theatre in Aurora, Colorado, in 2012 (12 deaths and 58 individuals with nonfatal gunshot wounds); or a church in Southerland Springs, Texas (26 deaths and 20 nonfatally wounded). The data for this study also counted the Virginia Tech mass shooting (32 deaths and 23 victims with nonfatal wounds) as three incidents as a result of the way that the SHR limits the number of victims to 11 in any given homicide incident. Another recent state-level study used an open-source database compiled by the publication *Mother Jones* and found no association between measures of gun ownership and gun law permissiveness and fatal mass shootings in public places (Lin, Fei, Barzman, & Hossain, 2018). The generally undescribed gun law permissiveness measure, however, seemed to be limited to concealed carry restrictions, and the *Mother Jones* database has been criticized for inconsistent application of inclusion/exclusion criteria and for missing some cases (Fox & Fridel, 2016).

Luca and colleagues estimated the effects of several state gun laws—CBC laws that extend background check requirements to private transfers, purchaser licensing laws, regulations over civilians carrying concealed weapons, bans of assault weapons or large-capacity magazines (LCMs)—and the probability that a four-fatality mass shooting occurred in a given state and year during 1989–2014 (Luca, Malhotra, & Poliquin, 2019). Unfortunately, the authors used linear regression models that violated model assumptions for binary outcomes and thus made the findings difficult to interpret.

Two recent studies, each using different data sources and different outcome measures for fatal mass shootings, drew different conclusions regarding the association between the federal ban of assault weapons and LCMs. Fox and Fridel (2016) used the SHR data to examine cases involving four or more firearm homicide victims and found no association between the incidence of fatal mass shootings and the presence of the federal ban of assault weapons and LCMs. It is curious that these researchers did not examine whether the ban influenced the number of persons shot in mass shootings because the characteristics of the banned products are relevant to how many shots can be fired in a short span of time. Indeed, recent studies have documented that fatal mass shootings committed with assault weapons and/or LCMs result in significantly more victims shot than is the case in such shootings which involved no assault weapons or LCMs (Klarevas, 2016; Koper, 2020, this issue; Koper, Johnson, Nichols, Ayers, & Mullins, 2018). DiMaggio and colleagues (2019) published a study in which they reported that during the period when the federal ban of assault weapons and LCMs was in place (1994–2004), fatal mass shootings were 70% less likely to occur. But this study had major limitations based on the data used and the lack of statistical controls for other law changes or social trends that might explain variation in mass shootings. The study used data on fatal public mass shootings with four or more fatalities for the years 1981 through 2017 that were collected by three open-source databases—*Mother Jones*, *Los Angeles Times*, and Stanford University. Inexplicably, the researchers only included cases in their analyses that appeared in all three sources and thereby excluded many incidents of fatal mass shootings. This limited their data to only 51 public mass shootings that presumably were the most widely publicized. The study did not examine variation by state and thus did not consider state gun laws nor did it control for other covariates other than linear trend. Gius (2015) estimated the effects of federal and state bans of assault weapons and LCMs with annual data from the SHR for the years 1982–2011 and found evidence that such bans were linked to lower rates of fatalities in mass shootings. Klarevas, Conner, and Hemenway (2019) found that LCM bans were associated with significantly fewer incidents of high-fatality (six or more victims) mass shootings and lower fatality rates for such shootings during the period 1990–2017. An important limitation of this study was that it did not consider the effects of any other type of firearm laws.

In-depth studies of the circumstances surrounding public mass shootings in the United States during 2000–2017 have found that armed civilians with concealed carry permits played a role in stopping mass

shootings while they are in progress in 5% of the incidents (ALERT & FBI, 2018; Blair & Schwieit, 2014). The presence of armed civilians could also potentially deter some attacks in public places. Conversely, because some mass shootings result from spontaneous responses to conflict, having more people with immediate access to a firearm could spur more mass shootings. The Violence Policy Center (2019) identified 33 incidents between May 2007 and January 2019 in which someone with a permit to carry a concealed firearm shot and killed three or more people in an incident. Prior studies designed to estimate the impact of reducing legal restrictions on civilian concealed gun carrying in public places have been plagued by methodological limitations and have found inconsistent relationships between the adoption of such laws and homicides (Crifasi et al., 2018; Donohue, Aneja, & Weber, 2019; Morral, 2017). As a result, there is great uncertainty about the impact of laws that reduce barriers to civilian gun carrying on fatal mass shootings.

1 | METHOD

1.1 | Data

This research relied on data obtained from the FBI's SHR, which includes information on the number of victims, the demographics of the offender(s) and victim(s), the weapon(s) used, some circumstances or perpetrator motives, and the relationship between the offender and the first victim. We limited our data set to incidents of homicide that occurred between 1984 and 2017, involved four or more victims (excluding any offender death), and involved a firearm of any type. We excluded any case that was coded as having a connection to gang or narcotic activity because one of our supplemental data sets excludes gang- or narcotic-related events. Other studies that have examined mass shooting frequency have excluded gang and narcotic incidents, so we excluded these incidents to adhere to the current literature (Klarevas, 2016; Lankford, 2016). We also created a variable that indicated whether a shooting involved a domestic relationship because some laws restrict firearm access based on history of domestic violence. We defined domestic relationships broadly, including any offender–victim family relationship, boyfriend/girlfriend, or ex-spouse. Importantly, the offender–victim relationship data in SHR is based on the relationship between the offender and the first victim recorded in the homicide report.

Because SHR data rely on voluntary law enforcement reporting, some homicide data is missing. In particular, exploratory analysis revealed that the SHR did not include several high-profile, high-casualty mass shootings including the 2012 Newtown, CT, school shooting; the 2012 Aurora, CO, movie theater shooting; and the 2017 Sutherland Springs, TX, church shooting. To remedy these and other omissions, we compared the SHR data with data on mass shootings collected by Stanford University (*Stanford Mass Shootings in America, courtesy of the Stanford Geospatial Center and Stanford Libraries*, n.d.) for the years 1984–2017 and the Gun Violence Archive for the years 2014–2017 (*Mass Shootings in 2017*, n.d.) and added any missing incidents to our data set.[2] We followed Zeoli et al. (2018) in excluding Florida, Kansas, Kentucky, Nebraska, and Montana from our analysis because of systemic Uniform Crime Reports (UCR)–SHR reporting issues over multiple years.

Data on gun laws were collected and coded using traditional legal research methods. We included several state-level statutes: concealed carry laws, handgun purchaser licensing laws that require either in-person application or fingerprinting, laws requiring point-of-sale background checks only, firearm prohibitions for subjects of domestic violence restraining orders that include ex parte orders, firearm prohibitions for subjects of domestic violence restraining orders that include dating partners in the

definition of domestic violence, firearm prohibitions for subjects of domestic violence restraining orders that do not include ex parte orders or dating partners, laws requiring surrender of all firearms by subjects of domestic violence restraining orders, firearm prohibitions for violent misdemeanants, assault weapon bans, and large-capacity magazine bans. Some of the legal data was obtained from prior work (Zeoli et al., 2018). We obtained any missing legal data from the Thomson Reuters Westlaw database. Using Westlaw, Hein Online, and Lexis Nexis, we tracked each state's statutory history to determine when each law was enacted. Each collected law was compared with existing publicly available databases of state gun laws (Everytown; Giffords; *State Firearm Laws*). Any conflicts between our data set and the databases was resolved by reevaluating the statutory or legislative text. Specific laws and the states and time periods in which they were in effect are presented in Table 1. For our analysis, we coded the laws using a binary 0–1 variable that was only equal to 1 in a year in which a given state law was in effect for at least half of the year.

Our demographic control variables included a commonly used proxy measurement of gun ownership (proportion of all suicides where the chosen method was a firearm), state unemployment rate, poverty rate, percent population identified as male, percent population identified as Black, percent married, percent divorced, percent military veteran, percent living in an Metropolitan Statistical Area, ethanol consumption per capita, religious adherence, percent with a high school diploma, the drug overdose rate (estimated by the rate of nonsuicide overdose deaths), and the proportion of the population aged 15–24 years. These variables were gathered from the U.S. Census Bureau (Census), the Centers for Disease Control and Prevention (CDC), the Bureau of Labor Statistics (BLS), the Religion and Congregation Membership Survey (ARDA), and the National Institute on Alcohol Abuse and Alcoholism (NIAAA, 2017). Missing years of demographic data were interpolated. These control variables were selected based on prior research on firearm homicide and suicide (Crifasi et al., 2015; Rudolph et al., 2015; Zeoli et al., 2018).

1.2 | Analysis

We used generalized linear models with a negative binomial distribution to conduct pooled time-series analyses of three dependent variables measured at the state-year level: domestic-linked mass shootings, non–domestic-linked mass shootings, and all mass shootings. All three are overdispersed count variables. In addition to analyzing incidents of fatal mass shootings, we also analyzed the number of victim fatalities in fatal mass shootings as an outcome variable. The models included state fixed effects, the law variables, and the sociodemographic covariates as well as linear and quadratic trend terms to control for unmeasured conditions that may have influenced fatal mass shootings during the study period. In addition to the full models with all covariates, we examined parsimonious models that limited the sociodemographic control variables with coefficients in the full model that had p values less than .10. All models used a negative binomial distribution with robust standard errors accounting for clustering by state and with overall state population as the exposure variable.

We also performed several sensitivity analyses. To provide a more flexible control for unmeasured national trends, we substituted year fixed effects for the linear and quadradic trend terms in our models. Prior work has suggested that LCM and assault weapon bans might phase in gradually because of pre-ban spikes in purchasing and production (Koper, Woods, & Roth, 2004). To examine this, we ran our models with state LCM bans and state and federal assault weapon bans coded to phase in gradually, starting with .2 in year 1 and increasing .2 per year until hitting 1 in year 5. To evaluate whether specific, high-profile mass shooting incidents might be leading to policy adoption, we ran our models without specific observations for the years just prior to policy implementation.

TABLE 1 Federal and state laws examined and dates those laws went into in effect or were repealed

			Private Transfer Laws		Prohibitions Related to Domestic Violence Restraining Orders (DVROs)			
State	Assault Weapon Ban	Large-Capacity Magazine Ban	Purchaser licensing with in-person or fingerprinting	Point-of-sale background check only	Final DVRO only	Includes ex parte orders	Includes dating partners	Includes surrender provision
Alabama					9/1/15			
Alaska							7/1/96	7/1/96
Arizona					7/20/96–7/21/97	7/21/97	9/30/09	7/20/96
Arkansas								
California	12/31/91	1/1/00		1/1/91		1/1/95	1/1/91	1/1/95
Colorado		7/1/13		7/1/13	7/1/13		2/26/94–11/30/98	7/1/13
Connecticut	7/1/94	4/4/13	10/1/95		10/1/94–10/1/99	10/1/16	10/1/99	10/1/94
Delaware				7/1/13		1/16/94	9/18/07	1/16/94
Georgia								
Hawaii			pre-1984		6/10/93–7/1/94	7/1/94	6/7/00	6/10/93
Idaho								
Illinois						1/1/10	1/1/96	1/1/96
Indiana				pre-1984–11/30/98			7/1/02	7/1/02
Iowa			pre-1984		7/1/10			7/1/10
Louisiana							8/1/14	
Maine					9/19/97–9/13/03	9/13/03		9/13/03
Maryland	10/1/13	8/1/94	10/1/13	10/1/96–10/1/13	10/1/96–10/1/09	10/1/09	10/1/15	10/1/96
Massachusetts	10/21/98	10/21/98	pre-1984			7/1/94	7/1/94	7/1/94
Michigan			pre-1984–12/18/12				4/1/96	
Minnesota							8/1/14	8/1/14

(Continues)

TABLE 1 (Continued)

State	Assault Weapon Ban	Large-Capacity Magazine Ban	Private Transfer Laws: Purchaser licensing with in-person or fingerprinting	Private Transfer Laws: Point-of-sale background check only	Prohibitions Related to Domestic Violence Restraining Orders (DVROs): Final DVRO only	Prohibitions Related to DVROs: Includes ex parte orders	Prohibitions Related to DVROs: Includes dating partners	Prohibitions Related to DVROs: Includes surrender provision
Mississippi								
Missouri			pre-1984–8/28/07					
Nevada				1/1/17			10/1/07	10/1/07
New Hampshire						1/1/00	1/1/00	1/1/00
New Jersey	5/1/90	5/1/90	pre-1984			11/11/91	8/11/94	8/11/94
New Mexico								
New York	11/1/00	11/1/00	pre-1984			11/1/96	7/21/08	11/1/96
North Carolina					12/1/95–12/1/97	12/1/03	12/1/97	12/1/03
North Dakota								
Ohio								
Oklahoma								
Oregon				8/9/15	1/1/16			
Pennsylvania				10/11/95		5/9/06	12/5/94	12/5/94
Rhode Island				pre-1984		7/1/17	7/1/05	7/1/05
South Carolina					6/4/15			
South Dakota								
Tennessee				5/10/94–11/1/98	7/1/09			7/1/09
Texas						1/1/08	9/1/01	
Utah						7/1/95		
Vermont							2/2/01	
Virginia						7/1/94		

(Continues)

TABLE 1 (Continued)

State	Assault Weapon Ban	Large-Capacity Magazine Ban	Private Transfer Laws: Purchaser licensing with in-person or fingerprinting	Private Transfer Laws: Point-of-sale background check only	Prohibitions Related to Domestic Violence Restraining Orders (DVROs): Final DVRO only	Includes ex parte orders	Includes dating partners	Includes surrender provision
Washington				12/4/14		7/1/94	7/23/95	7/1/94
West Virginia						4/14/01	6/2/98	
Wisconsin					4/1/96–7/30/02		7/30/02	4/1/96
Wyoming								

State	Concealed Carry Permitting Laws: No issue	May issue	Shall issue with discretion	Strict shall issue	Permitless carry	Violent Misdemeanor Prohibition
Alabama		pre-1984–8/1/13	8/1/13			9/1/15
Alaska	pre-1984–10/1/94			10/1/94–9/9/03	9/9/03	
Arizona	pre-1984–7/16/94			7/16/94–7/28/10	7/28/10	
Arkansas	pre-1984–7/27/94		7/27/94			
California		pre-1984				1/1/91
Colorado		pre-1984–5/17/03	5/17/03			
Connecticut		pre-1984				10/1/94
Delaware		pre-1984				
Georgia		pre-1984–8/25/89	8/25/89			
Hawaii		pre-1984				6/13/88
Idaho		pre-1984–7/1/90		7/1/90–7/1/16	7/1/16	
Illinois	pre-1984–1/5/14		1/5/14			1/1/96

(Continues)

TABLE 1 (Continued)

State	Concealed Carry Permitting Laws					Violent Misdemeanor Prohibition
	No issue	**May issue**	**Shall issue with discretion**	**Strict shall issue**	**Permitless carry**	
Indiana			pre-1984			
Iowa		pre-1984–1/1/11	1/1/11			
Louisiana	pre-1984–4/19/96			4/19/96		
Maine				pre-1984–10/15/15	10/15/15	
Maryland		pre-1984				10/1/96
Massachusetts		pre-1984				
Michigan		pre-1984–7/1/01		7/1/01		
Minnesota		pre-1984–5/28/03	5/28/03			8/1/03
Mississippi	pre-1984–7/1/91			7/1/91–4/15/16	4/15/16	
Missouri	pre-1984–2/26/04		2/26/04–1/1/17		1/1/17	
Nevada		pre-1984–10/1/95		10/1/95		
New Hampshire			pre-1984–2/22/17		2/22/17	
New Jersey		pre-1984				
New Mexico	pre-1984–1/1/04			1/1/04		
New York		pre-1984				pre-1984
North Carolina	pre-1984–12/1/95			12/1/95		

(Continues)

TABLE 1 (Continued)

State	Concealed Carry Permitting Laws					Violent Misdemeanor Prohibition
	No issue	May issue	Shall issue with discretion	Strict shall issue	Permitless carry	
North Dakota	pre-1984–8/1/85			8/1/85–8/1/17	8/1/17	4/15/85
Ohio	pre-1984–4/8/04			4/8/04		
Oklahoma	pre-1984–9/1/95			9/1/95		
Oregon		pre-1984–1/1/90	1/1/90			
Pennsylvania		pre-1984–6/17/89	6/17/89			
Rhode Island			pre-1984			
South Carolina		pre-1984–8/23/96		8/23/96		
South Dakota		pre-1984–7/1/85		7/1/85		
Tennessee	pre-1984–11/1/89	11/1/89–10/1/96		10/1/96		
Texas	pre-1984–1/1/96			1/1/96		
Utah		pre-1984–5/1/95	5/1/95			
Vermont					pre-1984	7/1/15
Virginia		pre-1984–7/1/95	7/1/95			
Washington				pre-1984		
West Virginia		pre-1984–7/7/89		7/7/89–5/24/16	5/24/16	
Wisconsin	pre-1984–11/1/11			11/1/11		
Wyoming		pre-1984–10/1/94	10/1/94–7/1/11		7/1/11	

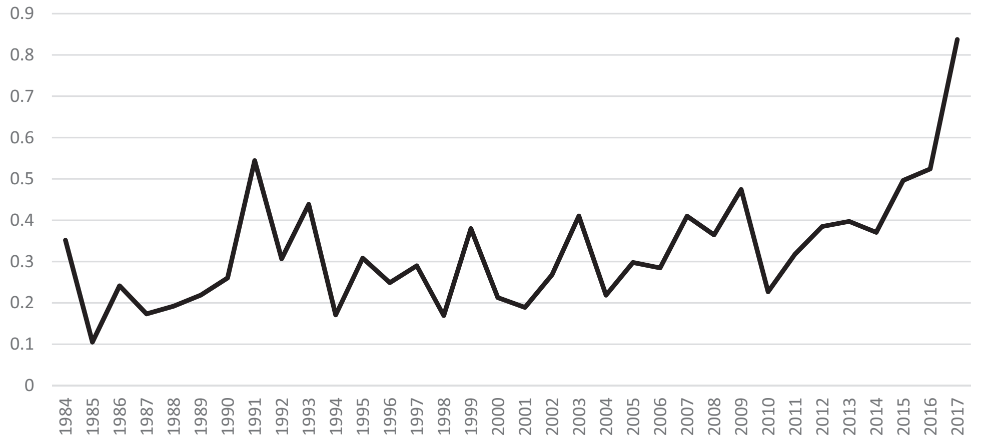

FIGURE 1 Victims in fatal mass shootings per 1 million population per year, 1984–2017

We also examined whether our findings changed when the cutoff for defining a fatal mass shooting was five or more victims and six or more victims. All models were estimated in Stata/IC 15.1 (StataCorp).

2 | RESULTS

We identified 604 mass shooting incidents involving four or more murdered victims that met our inclusion criteria (no gang- or drug-related shootings) during the 1984–2017 study period. There were 2,976 victims murdered in these incidents, 842 (28.3%) in domestic-related shootings, 2,057 (69.1%) victims in non–domestic-related shootings, and 77 victims in all shootings in which it was unclear whether the shooting was domestic related. The annual rate of mass shooting fatalities per 1 million population nationwide was .36 per 100,000 population and ranged from 0 in Delaware and Rhode Island to .88 in South Carolina (see Table A1 in the Appendix). This rate was stable through most of the study period, drifted upward during 2007–2014, before accelerating between 2014 and 2017 (Figure 1). The mean number of victim fatalities by gunfire per incident during the study period was 4.93; victim fatalities were somewhat higher during the years after the federal ban of assault weapons and LCMs expired compared with the decade during which the ban was in place (5.85 during 2005–2017 vs. 4.59 during 1995–2004; Figure 2). Most shootings had four to six victims (Figure 3). A list of descriptive statistics for independent variables can be found in Table 2.

The estimates from the full negative binomial models (Table 3) indicate that handgun purchaser licensing laws requiring in-person application with law enforcement or fingerprinting were associated with incidents of fatal mass shootings 56% lower than that of other states (internal rate of return [IRR] = 0.44, 95% confidence interval [CI] 0.26, 0.73). For LCM bans, the IRR estimate (0.52, 95% CI = 0.27, 0.98) indicates a 48% lower risk of fatal mass shootings associated with the policy. We found no evidence that concealed carry laws, assault weapons bans, prohibitions for domestic abusers and violent misdemeanants, or point-of-sale CBC laws were associated with the incidence of fatal mass shootings. In models in which the number of mass shooting victim fatalities was the outcome, handgun purchaser licensing was protective (IRR = 0.44, 95% CI 0.24, 0.82) and the point estimate for LCM bans suggests a large protective effect albeit with a wide confidence interval (IRR = 0.30, 95% CI .08, 1.10) that make inferences less certain.

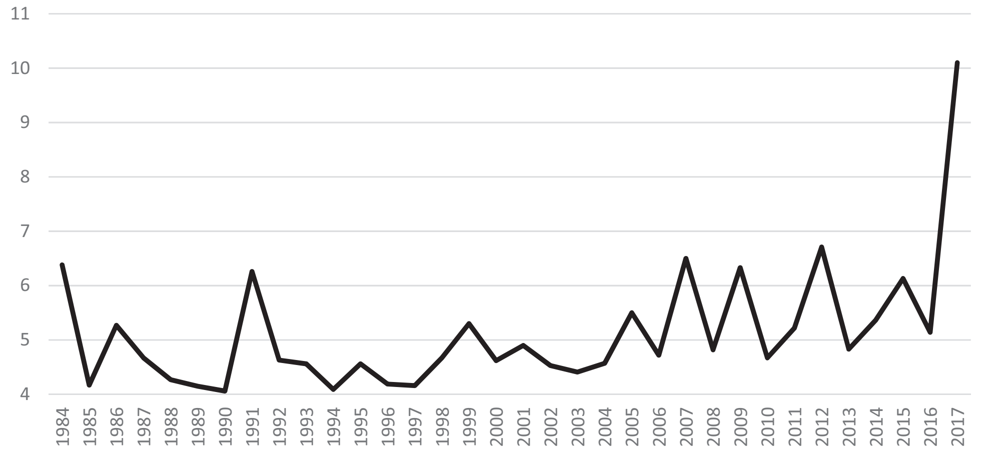

FIGURE 2 Mean number of victims murdered per incident in shootings involving 4+ victim fatalities, 1984–2017

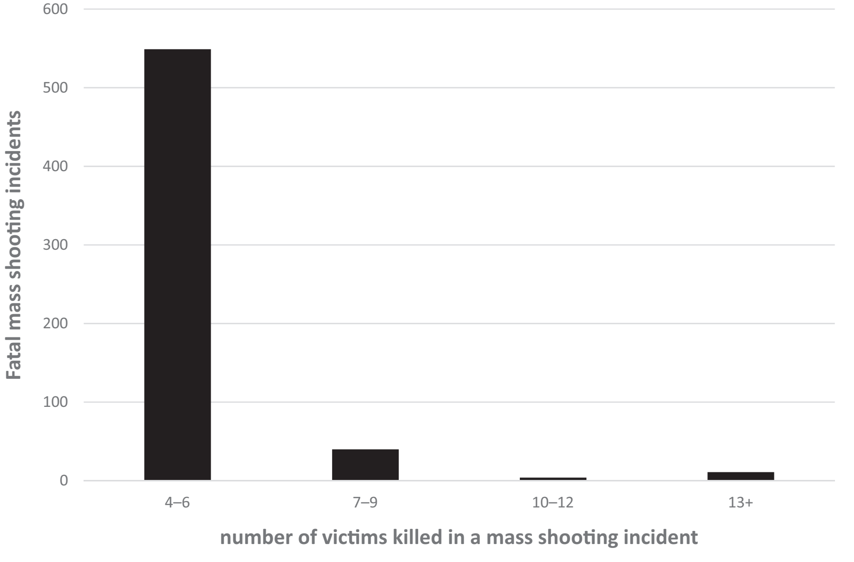

FIGURE 3 Number of incidents of fatal mass shootings by the number of victims killed, united states, 1984–2017

Models for the incidence of mass shootings with domestic or intimate partner violence links revealed no significant associations with laws prohibiting firearms for domestic violence abusers or violent misdemeanants, or purchaser licensing laws (Table 4). LCM bans, however, were associated with a 61% lower rate of domestic mass shootings (IRR = 0.39, 95% CI 0.21, 0.73). The association for LCM bans was somewhat stronger in models for the number of victim fatalities in mass shootings (IRR = 0.25, 95% CI 0.11, 0.59). CBC laws were associated with large increases in domestic mass shooting victim counts (IRR = 2.23, 95% CI 1.10, 4.51).

Purchaser licensing laws were associated with a 62% lower incidence of non–domestic-linked fatal mass shootings (IRR = 0.38, 95% CI 0.20, 0.70) in the full model (Table 5). If the proxy for gun ownership is left out of the model, the IRR is similar (IRR = 0.39, 95% CI 0.22, 0.67). LCM bans were

TABLE 2 Descriptive statistics for independent variables used in the analyses

Variable	Mean	Min	Max	SD
Concealed carry permits—May issue as reference No issue	.14	0	1	.35
Shall issue with discretion	.21	0	1	.41
Strict shall issue	.28	0	1	.45
Permitless	.05	0	1	.21
Purchaser licensing with discretion	.07	0	1	.25
Purchaser licensing in-person application/fingerprint required	.17	0	1	.37
Comprehensive background check—point of sale	.09	0	1	.28
DVRO firearm prohibition w/ final order, no dating partners	.04	0	1	.20
DVRO firearm prohibition includes ex parte	.22	0	1	.41
DVRO firearm prohibition includes dating partners	.27	0	1	.44
DVRO firearm prohibition surrender provision	.28	0	1	.45
Violent misdemeanor	.13	0	1	.34
Federal assault weapon ban	.29	0	1	.46
State assault weapon ban	.08	0	1	.26
Large-capacity magazine ban	.08	0	1	.27
Gun ownership (firearm suicides/all suicides)	.56	.13	.87	.14
Unemployment (%)	5.76	2.3	14.8	1.91
Percent in poverty	12.84	2.9	27.2	3.79
Percent male	49.16	47.63	52.71	.87
Percent Black	10.91	.28	38.29	9.77
Percent married	54.81	42.26	67.64	4.93
Percent divorced	10.31	4.78	16.54	2.03
Percent veteran	13.10	4.00	21.88	3.87
Percent living in MSA	70.09	14.94	100	19.94
Ethanol consumption per capita	2.40	1.23	5.10	.54
Religious adherence (%)	50.62	22.43	83.97	11.57
Percent Completed high school	83.30	62.59	92.8	5.87
Drug overdose rate	7.30	.14	55.26	6.55
Log proportion aged 15–24	–1.93	–2.15	–1.61	.09

Note. DVRO = domestic violence restraining order; MSA = Metropolitan Statistical Area; SD = standard deviation. Models also include state fixed effects, linear and quadratic time trend terms.
$^{*}p = .05$.

linked with a lower incidence of non–domestic-linked fatal mass shootings in the parsimonious model (IRR = .34, 95% CI .14, .81); however, the IRR estimate for LCM bans of .65 and was not statistically significant in the full model. None of the other firearm laws were associated with the incidence of non–domestic-linked fatal mass shootings.

2.1 | Sensitivity Analyses

The models that assumed gradual effects for bans of assault weapons and large capacity magazines produced somewhat different results (Tables A2–A4). The negative association between LCM bans

TABLE 3 Estimates for incident rate ratio for the incidence of fatal mass shootings

Variable	Incidents (n = 604) IRR	Incidents (n = 604) 95% CI	Victim Deaths (n = 2,976) IRR	Victim Deaths (n = 2,976) 95% CI
Concealed carry permits—May issue as reference No issue	.93	[.55, 1.58]	1.53	[.82, 2.85]
Shall issue with discretion	.91	[.51, 1.60]	1.14	[.60, 2.19]
Strict shall issue	1.28	[.72, 2.27]	1.44	[.70, 2.94]
Permitless	1.29	[.50, 3.29]	1.02	[.32, 3.28]
Purchaser licensing in-person application/fingerprint required	**.44***	[.26, .73]	**.43***	[.26, .73]
Comprehensive background check—point of sale	1.10	[.77, 1.58]	1.43	[.74, 2.77]
DVRO firearm prohibition w/ final order, no dating partners	.86	[.42, 1.77]	.72	[.33, 1.59]
DVRO firearm prohibition includes ex parte	1.10	[.76, 1.58]	1.13	[.71, 1.77]
DVRO firearm prohibition includes dating partners	.89	[.56, 1.42]	.91	[.50, 1.65]
DVRO firearm prohibition surrender provision	.76	[.50, 1.16]	.75	[.44, 1.27]
Violent misdemeanor	1.51	[.79, 2.89]	1.25	[.63, 2.46]
Federal assault weapon ban	.92	[.67, 1.26]	.96	[.63, 1.46]
State assault weapon ban	.71	[.34, 1.48]	1.11	[.30, 4.16]
Large-capacity magazine ban	**.52***	[.27, .98]	.30	[.08, 1.10]
Gun ownership	.15	[.00, 4.76]	.96	[.93, 1.00]
Unemployment	1.03	[.95, 1.10]	1.02	[.92, 1.13]
Percent in poverty	1.01	[.95, 1.07]	1.00	[.93, 1.07]
Percent male	.80	[.37, 1.70]	.84	[.36, 1.94]
Percent Black	1.07	[.91, 1.26]	1.18	[.96, 1.45]
Percent married	1.03	[.94, 1.13]	1.00	[.89, 1.11]
Percent divorced	1.03	[.80, 1.32]	.99	[.74, 1.32]
Percent veteran	**.86***	[.75, .99]	.92	[.78, 1.09]
Percent living in MSA	1.00	[.98, 1.03]	1.00	[.97, 1.02]
Ethanol consumption per capita	1.10	[.40, 3.03]	.80	[.24, 2.69]
Religious adherence	1.01	[.97, 1.06]	.99	[.93, 1.04]
Percent completed high school	1.05	[.98, 1.13]	1.06	[.97, 1.16]
Drug overdose rate	1.01	[.97, 1.05]	.99	[.95, 1.03]
Log proportion aged 15–24	**.06***	[.00, .99]	.99	[.95, 1.03]

Note. CI = confidence interval; DVRO = domestic violence restraining order; IRR = incident rate ratio; MSA = Metropolitan Statistical Area; SD = standard deviation. Models also include state fixed effects, linear and quadratic time trend terms.
* p = .05.

and total fatal mass shootings (IRR = 0.74, 95% CI 0.42, 1.31) and the number of victims killed in mass shootings (IRR = 0.38, 95% CI 0.10, 1.44) was no longer statistically significant in the full model, but it was associated with lower incidence in the parsimonious model for all fatal mass shootings (IRR = 0.54, 95% CI 0.29, 1.00). For domestic-linked mass shootings, LCM bans were associated with lower incidence in the parsimonious model for (IRR = 0.58, 95% CI 0.36, 0.94) and with fewer victim fatalities in the full model (IRR = 0.31, 95% CI 0.11, 0.86). Purchaser licensing laws were associated with lower incidence of total fatal mass shootings (IRR = 0.46, 95% CI 0.27, 0.77) and lower incidence rates for non–domestic-linked fatal mass shootings (IRR = 0.42, 95% CI 0.22, 0.77).

TABLE 4 Estimates for incident rate ratio for domestic-linked mass shootings

Variable	Incidents (n = 182) IRR	Incidents (n = 182) 95% CI	Victim Deaths (n = 842) IRR	Victim Deaths (n = 842) 95% CI
Concealed Carry Permit—May issue reference No issue	.66	[.26, 1.68]	.74	[.27, 2.08]
Shall issue w/discretion	.98	[.41, 2.34]	.81	[.33, 2.00]
Strict shall issue	.90	[.33, 2.46]	.78	[.25, 2.48]
Permitless	2.33	[.35, 15.70]	1.43	[.16, 13.21]
Purchaser licensing in-person application or fingerprint required	.93	[.39, 2.19]	1.43	[.60, 3.39]
Comprehensive background checks—point of sale	1.88	[.92, 3.85]	**2.22***	[1.10, 4.50]
DVRO prohibition—final orders, dating partner excluded	.89	[.31, 2.56]	.69	[.22, 2.13]
DVRO prohibition ex parte included	1.51	[.84, 2.71]	1.42	[.74, 2.74]
DVRO includes dating partners	.91	[.57, 1.43]	.80	[.50, 1.30]
DVRO surrender required	.85	[.45, 1.64]	.82	[.40, 1.67]
Violent misdemeanor prohibition	1.86	[.45, 7.69]	2.08	[.57, 7.60]
Federal assault weapons/LCM ban	.87	[.50, 1.51]	.84	[.46, 1.55]
State assault weapons ban	.40	[.14, 1.19]	.42	[.13, 1.32]
Large-capacity magazine ban	**.39***	[.21, .73]	**.25***	[.11, .59]
Gun ownership	.06	[.00, 8.9]	.96	[.89, 1.04]
Unemployment	1.05	[.91, 1.21]	1.09	[.92, 1.29]
Percent in poverty	1.01	[.89, 1.15]	1.00	[.87, 1.14]
Percent male	1.02	[.28, 3.68]	1.08	[.23, 5.03]
Percent Black	1.00	[.81, 1.24]	1.03	[.81, 1.30]
Percent married	.96	[.82, 1.13]	.97	[.82, 1.16]
Percent divorced	.90	[.61, 1.32]	.91	[.58, 1.43]
Percent veteran	1.00	[.83, 1.22]	1.08	[.89, 1.31]
Percent living in MSA	1.00	[.95, 1.05]	.98	[.93, 1.03]
Ethanol consumption per capita	.91	[.14, 6.00]	.79	[.11, 5.78]
Religious adherence	1.02	[.94, 1.10]	1.00	[.92, 1.08]
Percent completed high school	1.02	[.91, 1.14]	.99	[.88, 1.12]
Drug overdose rate	.98	[.92, 1.04]	.97	[.91, 1.04]
Log proportion aged 15–24	1.26	[.02, 95.3]	1.02	[.78, 1.34]

Note. CI = confidence interval; DVRO = domestic violence restraining order; IRR = incident rate ratio; MSA = Metropolitan Statistical Area; SD = standard deviation. Models also include state fixed effects, linear and quadratic time trend terms.
*p = .05.

When we used year fixed effects to account for unmeasured national trends in mass shootings, our point estimates for the gun law variables were similar to those in our primary models with linear and quadratic trend terms; however, the confidence intervals for the estimates expanded and the association between LCM bans and the incidence (.56, 95% CI .27, 1.16) and fatalities for all mass shootings (IRR = .37, 95% CI .11, 1.31) were no longer statistically significant at the .05 level (Table A5). Negative associations for LCM bans and the incidence and number of fatalities for domestic-linked mass shootings and negative associations between purchaser licensing and non–domestic-linked mass

TABLE 5 Estimates for models for mass shooting incidents not linked to domestic violence

Variable	Incidents (n = 401) IRR	95% CI	Victim Deaths (n = 2,057) IRR	95% CI
Concealed carry permit—may issue reference No issue	1.02	[.51, 2.05]	1.82	[.85, 3.90]
Shall issue with discretion	.84	[.38, 1.86]	1.19	[.50, 2.79]
Strict shall issue	1.52	[.86, 2.70]	1.83	[.89, 3.79]
Permitless	.68	[.26, 1.79]	1.10	[.25, 4.81]
Purchaser licensing in-person or fingerprint required	**.38***	[.21, .70]	**.35***	[.19, .63]
Comprehensive background check—point of sale	.84	[.48, 1.47]	1.09	[.44, 2.70]
DVRO prohibition—final orders, dating partner excluded	.88	[.32, 2.44]	.72	[.24, 2.19]
DVRO prohibition includes Ex Parte	1.02	[.53, 1.96]	1.17	[.59, 2.30]
DVRO prohibition Inc. Dating Partners	.88	[.44, 1.77]	.94	[.40, 2.19]
DVRO prohibition with Surrender Provision	.75	[.35, 1.60]	.84	[.35, 1.99]
Violent misdemeanor prohibition	1.32	[.65, 2.68]	.94	[.46, 1.91]
Federal assault weapon ban	.98	[.65, 1.46]	1.11	[.67, 1.85]
State assault weapon ban	.73	[.31, 1.72]	1.01	[.25, 4.11]
Large capacity magazine ban	.65	[.26, 1.63]	.43	[.10, 1.81]
Gun ownership	.77	[.01, 47.8]	.97	[.93, 1.02]
Unemployment	1.04	[.97, 1.11]	1.02	[.93, 1.12]
Percent in poverty	1.00	[.93, 1.07]	.98	[.90, 1.07]
Percent male	.67	[.26, 1.68]	.66	[.24, 1.81]
Percent Black	1.08	[.87, 1.33]	1.26	[.93, 1.69]
Percent married	1.06	[.92, 1.22]	.98	[.84, 1.14]
Percent divorced	1.10	[.77, 1.56]	.94	[.64, 1.38]
Percent Veteran	**.79***	[.66, .96]	.89	[.70, 1.13]
Percent living in MSA	1.01	[.98, 1.05]	1.01	[.97, 1.06]
Ethanol consumption per capita	1.20	[.26, 5.50]	.93	[.15, 5.78]
Religious adherence	1.01	[.95, 1.08]	.99	[.91, 1.07]
Percent completed high school	1.05	[.94, 1.18]	1.09	[.96, 1.23]
Drug overdose rate	1.03	[.99, 1.08]	1.01	[.96, 1.06]
Log proportion aged 15–24	.02	[.00, 1.46]	.78	[.53, 1.15]

Note. CI = confidence interval; DVRO = domestic violence restraining order; IRR = incident rate ratio; MSA = Metropolitan Statistical Area; SD = standard deviation. Models also include state fixed effects, linear and quadratic time trend terms.
*p = .05.

shootings were consistent with our primary models (Tables A6–A7). When we used Poisson fixed-effects regression models, our estimates for the association between the firearm laws of interest and fatal mass shootings were consistent with the estimates in our primary models (Tables A8-A10).

To evaluate whether particularly fatal mass shootings led to passage of the policies at interest, we conducted an analysis that omitted certain observations. We determined that, after a mass shooting with 10 or more fatalities, only two states adopted a law that showed a statistically significant effect in our main models: Connecticut and Colorado both adopted LCM bans after major mass shootings in 2012. We omitted the 2012 observations for these two states and repeated our analysis. When these

observations were omitted, the point estimate for purchaser licensing was similar to our main model of all mass shooting incidents (IRR = .40, 95% CI .23, .69; Table A11) and fatalities (IRR = .33, 95% CI .19, .59). Similarly purchaser licensing was associated with reductions in non–domestic-linked mass shootings (IRR = .38, 95% CI .20, .70; Table A13) and fatalities (IRR = .34, 95% CI .18, .62). For all mass shootings, LCM bans estimates were similar to our primary models but no longer statistically significant for incidents (IRR = .56, 95% CI .30, 1.03; Tale A11) and fatalities (IRR = .40, 95% CI .14, 1.14). LCM bans were statistically significant and protective for domestic-linked mass shooting incidents (IRR = .46, 95% CI .23, .89; Table A12) and fatalities (IRR = .45, 95% CI .22, .91).

In the models using different victim fatality thresholds for mass-shootings (five and six victims), the data were too sparse to stratify by domestic violence link. When mass shootings were limited to those with five or more victims (n = 198 shootings), LCM bans were associated with an 80% lower incidence in the full model (IRR = .20, 95% CI .06, .67; Table A14). Although the point estimate for purchaser licensing laws was similar to that for the models with four victim fatality thresholds, it was not statistically significant (IRR = .52, 95% CI .15, 1.83). The estimate for No Issue concealed carry permit laws did change dramatically with the five-fatality threshold and was associated with much higher incidence of fatal mass shootings (IRR = 4.14, 95% CI 1.57, 10.87; Table A14). No Issue concealed carry laws no longer exist, however, as every state now allows for some form of civilian concealed carry. Similarly, when mass shootings were limited to those with six or more victims (Table A15), LCM bans were associated with an 87% lower incidence in the full model (IRR = .14, 95% CI .03, .70) and purchaser licensing laws were not associated with any change.

3 | DISCUSSION

The rate at which Americans are murdered in mass shootings has increased in recent years. For decades, horrific mass shootings have prompted intense political debates about whether such incidents can be prevented and what would be the most effective policy responses. Prior research on the effects of firearm policies on fatal mass shootings has important limitations, leaving questions about the effectiveness of strengthened gun regulations such as comprehensive background checks or policies that have been implemented to encourage more civilian gun carrying in public places.

The findings of this study suggest that the most common policy prescriptions offered by advocates on each side of the debate over gun control—comprehensive background checks and assault weapons bans on one side and so-called “Right to Carry” laws reducing restrictions on civilian concealed carry of firearms on the other side—do not seem to be associated with the incidence of fatal mass shootings. Twenty-eight percent of the shootings in this study had some connection to domestic violence, yet we found no evidence that laws designed to keep firearms from perpetrators of domestic violence have affected mass shootings connected to domestic violence. This is somewhat surprising given prior research demonstrating that laws prohibiting persons under domestic violence restraining orders from possessing firearms or with prior convictions for violent misdemeanors were associated with reduced intimate partner homicides (Zeoli et al., 2018).

This study identified two policies associated with reductions in fatal mass shootings—laws requiring firearm purchasers or owners to acquire a license that involves in-person application and/or fingerprinting of applicants and state laws banning the purchase of LCMs or ammunition-feeding devices for semiautomatic firearms. The size of the estimated protective effects of these two policies are striking, although there are large confidence intervals. Firearm purchaser or owner licensing laws have been shown to reduce firearm homicides (Crifasi et al., 2018; Hasegawa, Small, & Webster, 2019; Rudolph et al., 2015; Webster, Crifasi, & Vernick, 2014) and suicides (Crifasi et al., 2015); thus, it

is plausible that these laws reduce firearm availability to individuals who are at risk of committing many forms of lethal violence including multivictim fatal shootings. States with licensing requirements for firearm purchasers typically review broader types of data to identify conditions that prohibit firearm possession and use fingerprints to identify individuals with criminal histories rather than rely solely on biographical information provided by the applicant. In addition, rigorous firearm purchaser licensing may also reduce illegal straw sales and other types of diversion of guns for criminal use (Crifasi, Buggs, Choksy, & Webster, 2017).

Assault rifles are commonly used in mass shootings with the most casualties, and certain design features of these weapons plausibly facilitate the ability of an assailant to rapidly shoot many rounds (e.g., barrel shrouds and pistol grips). But the capacity of the ammunition-feeding device and the ability to quickly reload may be the most relevant feature of firearms that influence the incidence and outcomes of mass shootings. Furthermore, most mass shootings do not involve assault rifles, but many involve the use of LCMs. This may explain why we found that LCM bans were associated with significant reductions in the incidence of fatal mass shootings but that bans on assault weapons had no clear effects on either the incidence of mass shootings or on the incidence of victim fatalities from mass shootings. Studies that have collected detailed data on the specific firearms used in fatal mass shootings show that firearms with LCMs are used roughly twice as frequently as firearms identified as assault weapons. In the Koper et al. (2018) study of mass shootings with four or more victim fatalities during 2009–2016, 19% involved firearms with an LCM and 10% involved firearm models classified as assault weapons. Additionally, Klarevas (2016) found that, during 2006–2015 (after the federal ban expired), 67% of mass shootings with six or more victim fatalities involved the use of an LCM versus 26% with an assault weapon model. Based on the data from Koper (2020), Koper et al. (2018), and Klarevas (2016), our point estimates may be somewhat higher than would be plausible based on the prevalence of LCM use in fatal public mass shootings, although the confidence intervals for these estimates are wide and encompass the estimates of the prevalence of use of LCMs in fatal mass shootings. Also, Koper (2013) found no evidence of decreased use of LCMs in the years after the federal ban in data from four cities that collected such data. This suggests that the supply of pre-ban LCMs was plentiful and that LCMs bans may take years to sufficiently reduce their availability for criminal misuse. Yet our models estimating gradual effects of state LCM bans showed weaker law effects than did the models assuming immediate effects. Passage of LCM bans may coincide with unmeasured factors related to protection against fatal mass shootings other than the comprehensive list of firearm laws examined here. Regardless, there is a clear functional link between LCMs and the ability of a shooter to take more lives. Our estimates of LCM ban impacts show the largest protective effects on high-fatality count shootings and on the number of victims murdered in mass shootings, and the point estimates are large in all model specifications.

It should be noted that the federal assault weapons ban and some state bans of assault weapons have resulted in gun manufacturers making slight alterations in the characteristics of weapon models that are banned. These newer models, assault weapons that were grandfathered by the bans, and the ability to purchase components of assault weapons online provide substitutes for the banned firearms for individuals considering carrying out acts of mass violence. LCM bans may be less likely to result in acquisition of equivalent substitutes as is the case for assault weapon bans.

There are limitations to this study that relate to the lack of systematic data at the state level on determinants of mass shootings that would aid in the modeling of state-level trends of rare events. We drew from prior research on factors associated with state-level rates of homicides and suicides. Mass shootings involve a very small proportion of such events, however, and the conditions that facilitate or suppress lethal violence overall may not explain rare and especially lethal mass shooting events. In addition, this study was not designed to fully explore the relationship between assault weapon bans and their

impact on fatal mass shootings. We did not examine, for example, whether the bans influenced the incidence of assault weapons being used in mass shootings because such data are not available for all fatal mass shootings. We also only examined fatal mass shootings, in which the number of fatalities rather than casualties determined whether an incident was included in the analysis. Booty, O'Dwyer, Webster, McCourt, and Crifasi (2019) have raised the issue of inconsistencies in mass shooting databases that define "mass shooting" differently, and we acknowledge that our results are influenced by the definition that we have chosen.

Despite these limitations, our estimates of the effects of state and federal gun laws on fatal mass shootings are mainly robust to different modeling assumptions and consistent with other research findings. Firearm purchaser licensing requirements are likely to reduce overall firearm availability within a state as well as reduce firearm availability to high-risk individuals. This study provides evidence that firearm purchaser or ownership licensing with fingerprinting reduce the risk of fatal mass shootings in addition to firearm homicides more broadly. LCM bans also seem to reduce the incidence of fatal mass shootings and the number of fatalities in mass shootings. Policy makers should consider these findings when crafting proposals to reduce deaths from mass shootings.

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ENDNOTES

[1] The researchers used *Traveler's Guide to the Firearms Laws of the Fifty States* that provides annual ratings for the restrictiveness–permissiveness scale of U.S. gun laws for each state based on assessments of legal professionals who represent gun owners in legal cases. This publication gives a rating between 0 (completely restrictive) and 100 (completely permissive).

[2] *Stanford Mass Shootings in America* collected data on incidents with three or more shooting casualties in a public place, excluding incidents related to gang or narcotic involvement; this data source ceased data collection in early 2016. The Gun Violence Archive (GVA) is a publicly available data source that collects information on incidents that had four or more shooting casualties, but a search query can restrict information to four or more fatalities. Twenty-three incidents were added from Stanford, and 10 incidents were added from GVA.

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APPENDIX

TABLE A1 Mean annual mass shooting rate and fatality rate by state

State	All Fatal Mass Shootings		Domestic-Linked Mass Shootings		Non–Domestic-Linked Mass Shootings	
	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population
Alabama	.04	.21	.01	.09	.02	.08
Alaska	.06	.40	.00	.00	.06	.40
Arizona	.11	.53	.03	.13	.07	.33
Arkansas	.13	.69	.02	.15	.11	.54
California	.06	.32	.03	.13	.03	.19
Colorado	.07	.39	.01	.05	.05	.31
Connecticut	.06	.48	.02	.26	.04	.22
Delaware	.00	.00	.00	.00	.00	.00
Georgia	.06	.28	.02	.08	.04	.20
Hawaii	.05	.25	.03	.10	.02	.15
Idaho	.09	.40	.03	.12	.06	.28
Illinois	.05	.22	.01	.03	.03	.17
Indiana	.09	.40	.04	.16	.06	.24
Iowa	.02	.10	.01	.05	.00	.00
Louisiana	.11	.46	.02	.09	.09	.37
Maine	.08	.30	.05	.20	.02	.10
Maryland	.04	.17	.02	.09	.02	.09
Massachusetts	.02	.09	.005	.02	.01	.07

(Continues)

TABLE A1 (Continued)

	All Fatal Mass Shootings		Domestic-Linked Mass Shootings		Non–Domestic-Linked Mass Shootings	
State	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Mean Annual Rate of Mass Shootings per 1 Million Population	Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population
Michigan	.11	.46	.03	.14	.07	.32
Minnesota	.03	.15	.01	.02	.02	.08
Mississippi	.09	.43	.00	.00	.07	.43
Missouri	.08	.35	.02	.07	.06	.28
Nevada	.08	.86	.03	.13	.05	.73
New Hampshire	.03	.12	.00	.00	.03	.12
New Jersey	.03	.11	.01	.03	.02	.08
New Mexico	.12	.59	.06	.29	.06	.30
New York	.05	.24	.01	.03	.04	.21
North Carolina	.11	.46	.01	.03	.10	.43
North Dakota	.14	.54	.14	.54	.00	.00
Ohio	.07	.29	.02	.08	.05	.21
Oklahoma	.08	.42	.03	.16	.04	.26
Oregon	.06	.30	.04	.17	.01	.03
Pennsylvania	.04	.19	.02	.07	.02	.12
Rhode Island	.00	.00	.00	.00	.00	.00

(Continues)

TABLE A1 (Continued)

State	All Fatal Mass Shootings: Mean Annual Rate of Mass Shootings per 1 Million Population	All Fatal Mass Shootings: Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Domestic-Linked Mass Shootings: Mean Annual Rate of Mass Shootings per 1 Million Population	Domestic-Linked Mass Shootings: Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population	Non–Domestic-Linked Mass Shootings: Mean Annual Rate of Mass Shootings per 1 Million Population	Non–Domestic-Linked Mass Shootings: Mean Annual Rate of Fatalities from Mass Shootings per 1 Million Population
South Carolina	.18	.88	.05	.20	.14	.68
South Dakota	.08	.34	.08	.34	.00	.00
Tennessee	.07	.29	.02	.07	.05	.20
Texas	.09	.47	.02	.11	.06	.34
Utah	.07	.40	.04	.19	.04	.21
Vermont	.10	.38	.00	.00	.10	.38
Virginia	.08	.48	.03	.13	.06	.35
Washington	.08	.38	.03	.12	.05	.26
West Virginia	.14	.64	.08	.34	.06	.30
Wisconsin	.04	.24	.01	.06	.03	.15
Wyoming	.12	.47	.12	.47	.00	.00
Overall	.07	.36	.03	.12	.04	.23

TABLE A2 Estimates for incident rate ratios for all fatal mass shootings using gradual assault weapon and LCM ban variables

Variable	All Fatal Mass Shooting Incidents (n = 604 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in All Fatal Mass Shootings (n = 2,976 fatalities) IRR (IRR)	95% CI (95% CI)
Concealed carry permits—may issue as reference	.94	[.55, 1.59]	1.53	[.83, 2.84]
No issue	(.97)	(.58, 1.63)	(1.45)	(.78, 2.68)
Shall issue with discretion	.95	[.54, 1.69]	1.15	[.59, 2.22]
	(.88)	(.50, 1.55)	(1.08)	(.54, 2.18)
Strict shall issue	1.34	[.75, 2.39	1.46	[.71, 2.98]
	(1.20)	(.72, 1.99)]	(1.36)	(.75, 2.47)
Permitless	1.35	[.52, 3.51]	1.02	[.31, 3.36]
	(1.24)	(.50, 3.03)	(.95)	(.30, 3.07)
Purchaser licensing[b]	**.46***	[.27, .77]	**.44***	[.24, .82]
	(.50)	(.34, .73)	(.62)	(.35, 1.07)
Comprehensive background check—point of sale	1.08	[.75, 1.55]	1.42	[.73, 2.79]
	(1.12)	(.78, 1.62)	(1.57)	(.72, 3.43)
DVRO firearm prohibition no dating partners	.83	[.40, 1.72]	.70	[.31, 1.62]
	(.94)	(.43, 2.04)	(.65)	(.30, 1.42)
DVRO firearm prohibition includes ex parte	1.08	[.74, 1.57]	1.10	[.69, 1.76]
	(1.04)	(.68, 1.57)	(.98)	(.59, 1.63)
DVRO firearm prohibition Includes dating partners	.93	[.58, 1.50]	.94	[.51, 1.70]
	(.89)	(.55, 1.42)	(.90)	(.50, 1.63)
DVRO firearm prohibition surrender provision	.75	[.48, 1.15]	.74	[.43, 1.25]
	(.77)	(.48, 1.25)	(.84)	(.48, 1.46)
Violent misdemeanor	1.50	[.82, 2.73]	1.30	[.67, 2.54]
	(1.48)	(.77, 2.84)	(1.30)	(.59, 2.87)
Federal assault weapon ban (gradual)	.95	[.70, 1.29]	1.02	[.65, 1.60]
	(.96)	(.70, 1.32)	(1.06)	(.70, 1.60)
State assault weapon ban (gradual)	.64	[.35, 1.18]	1.01	[.29, 3.47]
	(.66)	(.30, 1.48)	(.90)	(.21, 3.76)
Large-capacity magazine ban (gradual)	.74	[.42, 1.31]	.38	[.10, 1.44]
	(.54)	(.29, 1.00)	(.40)	(.10, 1.60)
Gun ownership	.98	[.95, 1.02]	.96	[.93, 1.00]
Unemployment	1.02	[.95, 1.10]	1.02	[.92, 1.13]
Percent in poverty	1.01	[.95, 1.07]	1.00	[.93, 1.07]
Percent male	.84	[.39, 1.78]	.85	[.37, 1.95]
Percent Black	1.07	[.91, 1.26]	1.19	[.96, 1.46]
Percent married	1.02	[.93, 1.13]	.99	[.88, 1.11]
Percent divorced	1.04	[.80, 1.33]	.99	[.74, 1.32]

(Continues)

TABLE A2 (Continued)

Variable	All Fatal Mass Shooting Incidents (n = 604 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in All Fatal Mass Shootings (n = 2,976 fatalities) IRR (IRR)	95% CI (95% CI)
Percent veteran	**.87***	[.76, .99]	.94	[.79, 1.10]
Percent living in MSA	1.00	[.98, 1.03]	1.00	[.97, 1.03]
Ethanol consumption per capita	1.13	[.42, 3.02]	.82	[.26, 2.64]
Religious adherence	1.02	[.97, 1.06]	.99	[.93, 1.04]
Percent completed high school	1.06	[.98, 1.14]	1.06	[.98, 1.16]
Drug overdose rate (per 100,000)	1.01	[.97, 1.05]	.99	[.95, 1.03]
Percent aged 15–24	.84	[.69, 1.02]	.88	[.71, 1.09]
Linear time trend	.91	[.80, 1.04]	.90	[.77, 1.04]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]

[a]Parsimonious model results.

[b]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*p = .05.

TABLE A3 Estimates for incident rate ratios for domestic-linked fatal mass shootings using gradual assault weapon and LCM ban variables

Variable	Domestic-Linked Fatal Mass Shooting incidents (*n* = 182 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in Domestic-Linked Mass Shootings (*n* = 842 fatalities) IRR (IRR)	95% CI (95% CI)
Concealed carry permit—may issue reference	.69	[.28, 1.74]	.80	[.29, 2.16]
No issue	(.67)	(.30, 1.51)	(.76)	(.31, 1.87)
Shall issue w/ discretion	1.02	[.42, 2.48]	.83	[.33, 2.07]
	(1.04)	(.46, 2.37)	(.89)	(.37, 2.14)
Strict shall issue	.94	[.35, 2.55]	.82	[.27, 2.55]
	(.96)	(.40, 2.28)	(.91)	(.33, 2.49)
Permitless	2.32	[.34, 15.75]	1.45	[.16, 13.37]
	(1.98)	(.33, 12.01)	(1.37)	(.16, 12.03)
Purchaser licensing[b]	.89	[.34, 2.37]	1.23	[.44, 3.42]
	(.80)	(.33, 1.93)	(1.53)	(.63, 3.77)
Comprehensive background checks—point of sale	1.79	[.89, 3.59]	**2.07***	[1.03, 4.17]
	(1.77)	(.90, 3.48)	**(2.20)***	(1.12, 4.32)
DVRO prohibition—final orders, dating partner excluded	.84	[.29, 2.45]	.66	[.21, 2.11]
	(.79)	(.33, 1.88)	(.49)	(.20, 1.22)
DVRO prohibition ex parte included	1.46	[.83, 2.58]	1.36	[.71, 2.61]
	(1.47)	(.85, 2.57)	(1.24)	(.63, 2.41)
DVRO includes dating partners	.93	[.59, 1.47]	.83	[.52, 1.33]
	(.89)	(.55, 1.45)	(.79)	(.46, 1.35)
DVRO surrender required	.82	[.42, 1.60]	.77	[.37, 1.60]
	(.85)	(.46, 1.58)	(.90)	(.45, 1.81)
Violent misdemeanor prohibition	1.61	[.45, 5.83]	1.87	[.57, 6.12]
	(1.89)	(.56, 6.37)	(2.15)	(.65, 7.14)
Federal assault weapons/LCM ban (gradual)	1.28	[.66, 2.48]	1.25	[.60, 2.59]
	(.93)	(.58, 1.51)	(.85)	(.49, 1.48)
State assault weapons ban (gradual)	.50	[.17, 1.43]	.62	[.19, 2.04]
	(.51)	(.19, 1.36)	(.68)	(.20, 2.33)
Large-capacity magazine ban (gradual)	.52	[.26, 1.02]	**.31***	[.11, .86]
	(.58)*	(.36, .94)	(.37)	(.13, 1.11)
Gun ownership	.97	[.90, 1.02]	.97	[.89, 1.04]
Unemployment	1.05	[.91, 1.22]	1.10	[.93, 1.30]
Percent in poverty	1.01	[.89, 1.15]	1.00	[.88, 1.14]
Percent male	.96	[.27, 3.48]	1.01	[.22, 4.67]
Percent Black	1.02	[.82, 1.28]	1.06	[.83, 1.34]
Percent married	.91	[.77, 1.08]	.92	[.76, 1.11]

(Continues)

TABLE A3 (Continued)

Variable	Domestic-Linked Fatal Mass Shooting incidents (n = 182 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in Domestic-Linked Mass Shootings (n = 842 fatalities) IRR (IRR)	95% CI (95% CI)
Percent divorced	.86	[.59, 1.27]	.88	[.56, 1.38]
Percent veteran	1.05	[.88, 1.24]	1.13	[.94, 1.36]
Percent living in MSA	1.00	[.95, 1.05]	.98	[.93, 1.03]
Ethanol consumption per capita	1.24	[.20, 7.88]	1.12	[.16, 7.90]
Religious adherence	1.02	[.94, 1.10]	1.00	[.93, 1.08]
Percent completed high school	1.01	[.91, 1.13]	.98	[.87, 1.10]
Drug overdose rate	.98	[.92, 1.04]	.97	[.91, 1.04]
Percent aged 15–24	1.00	[.74, 1.34]	1.01	[.75, 1.34]
Linear time trend	.97	[.77, 1.21]	1.00	[.79, 1.26]
Quadratic time trend	1.00	[1.00, 1.01]	1.00	[1.00, 1.01]

[a]Parsimonious model results.

[b]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*p = .05.

TABLE A4 Estimates for incident rate ratios for non–domestic-linked fatal mass shootings using gradual assault weapon And LCM ban variables

Variable	Non–Domestic-Linked Fatal Mass Shooting incidents (n = 401 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in Non–Domestic-Linked Mass Shootings (n = 2,057 fatalities) IRR (IRR)	95% CI (95% CI)
Concealed carry permit—may issue reference	1.01	[.50, 2.01]	1.78	[.84, 3.80]
No issue	(1.12)	(.55, 2.30)	(1.74)	(.82, 3.68)
Shall issue w/ discretion	.91	[.41, 2.02]	1.20	[.50, 2.89]
	(.81)	(.36, 1.83)	(1.00)	(.41, 2.43)
Strict shall issue	1.66	[.95, 2.92]	1.85	[.90, 3.83]
	(1.43)	(.87, 2.35)	(1.60)	(.88, 2.93)
Permitless	.75	[.28, 2.04]	1.12	[.25, 5.09]
	(.71)	(.27, 1.87)	(1.02)	(.22, 4.73)
Purchaser licensing[b]	**.42***	[.22, .77]	**.38***	[.20, .73]
	(.43)*	(.25, .72)	**(.48)***	(.26, .91)
Comprehensive background checks—point of sale	.81	[.46, 1.45]	1.07	[.43, 2.68]
	(.86)	(.48, 1.54)	(1.27)	(.42, 3.87)
DVRO prohibition—final orders, dating partner excluded	.84	[.30, 2.39]	.71	[.23, 2.22]
	(1.07)	(.34, 3.37)	(.78)	(.24, 2.57)
DVRO prohibition ex parte included	1.01	[.53, 1.94]	1.16	[.59, 2.30]
	(.94)	(.43, 2.03)	(1.09)	(.50, 2.35)
DVRO includes dating partners	.94	[.47, 1.89]	.97	[.41, 2.29]
	(.86)	(.43, 1.72)	(.91)	(.40, 2.08)
DVRO surrender required	.75	[.35, 1.60]	.83	[.35, 1.98]
	(.78)	(.33, 1.86)	(.91)	(.37, 2.26)
Violent misdemeanor prohibition	1.35	[.69, 2.67]	1.02	[.50, 2.07]
	(1.18)	(.57, 2.46)	(.90)	(.38, 2.15)
Federal assault weapons/LCM ban (gradual)	.86	[.59, 1.27]	1.08	[.62, 1.87]
	(.95)	(.66, 1.38)	(1.15)	(.71, 1.86)
State assault weapons ban (gradual)	.58	[.25, 1.33]	.67	[.17, 2.70]
	(.69)	(.27, 1.78)	(.67)	(.15, 2.90)
Large-capacity magazine ban (gradual)	1.10	[.47, 2.56]	.67	[.16, 2.76]
	(.50)	(.23, 1.09)	(.44)	(.11, 1.75)
Gun ownership	1.00	[.96, 1.04]	.97	[.93, 1.02]
Unemployment	1.03	[.96, 1.10]	1.02	[.93, 1.11]
Percent in poverty	1.00	[.93, 1.07]	.98	[.91, 1.07]
Percent male	.74	[.29, 1.86]	.68	[.25, 1.83]
Percent Black	1.08	[.88, 1.32]	1.25	[.93, 1.69]
Percent married	1.07	[.92, 1.24]	.98	[.83, 1.15]

(Continues)

TABLE A4 (Continued)

Variable	Non–Domestic-Linked Fatal Mass Shooting incidents (n = 401 shootings) IRR (IRR[a])	95% CI (95% CI[a])	Fatalities in Non–Domestic-Linked Mass Shootings (n = 2,057 fatalities) IRR (IRR)	95% CI (95% CI)
Percent divorced	1.13	[.79, 1.60]	.94	[.64, 1.38]
Percent veteran	**.79***	[.66, .95]	.89	[.70, 1.12]
Percent living in MSA	1.02	[.98, 1.05]	1.01	[.97, 1.06]
Ethanol consumption per capita	1.09	[.25, 4.76]	.88	[.15, 5.13]
Religious adherence	1.02	[.96, 1.08]	.99	[.91, 1.07]
Percent completed high school	1.07	[.95, 1.19]	1.10	[.97, 1.24]
Drug overdose rate	1.04	[1.00, 1.08]	1.01	[.96, 1.06]
Percent aged 15–24	.78	[.56, 1.07]	.78	[.53, 1.15]
Linear time trend	.90	[.77, 1.05]	.88	[.73, 1.05]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.01]

[a]Parsimonious model results.

[b]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*p = .05.

TABLE A5 Estimates for incident rate ratios for all fatal mass shootings (>3 victim fatalities), using year fixed effects

Variable	All Fatal Mass Shooting Incidents (n = 604 shootings) IRR	95% CI	Fatalities in All Fatal Mass Shootings (n = 2, 976 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.88	[.52, 1.48]	1.31	[.74, 2.32]
Shall issue w/ discretion	.83	[.47, 1.47]	.98	[.49, 1.95]
Strict shall issue	1.31	[.72, 2.39]	1.38	[.67, 2.84]
Permitless	1.21	[.49, 3.01]	.86	[.27, 2.73]
Purchaser licensing[a]	**.43***	[.26, .70]	**.44***	[.26, .75]
Comprehensive background checks—point of sale	1.00	[.69, 1.44]	1.16	[.63, 2.12]
DVRO prohibition—final orders, dating partner excluded	.94	[.46, 1.91]	.80	[.34, 1.85]
DVRO prohibition ex parte included	1.28	[.86, 1.90]	1.38	[.84, 2.25]
DVRO includes dating partners	.91	[.54, 1.51]	.92	[.48, 1.76]
DVRO surrender required	.69	[.45, 1.04]	.65	[.38, 1.10]
Violent misdemeanor prohibition	1.54	[.81, 2.95]	1.33	[.68, 2.59]
Federal assault weapons/LCM ban (gradual)	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]
State assault weapons ban (gradual)	.60	[.27, 1.35]	.84	[.23, 3.08]
Large-capacity magazine ban (gradual)	.56	[.27, 1.16]	.37	[.11, 1.31]
Gun ownership	.97	[.93, 1.01]	.96	[.92, 1.01]
Unemployment	1.08	[.96, 1.22]	1.06	[.91, 1.25]
Percent in poverty	1.01	[.94, 1.07]	.99	[.92, 1.07]
Percent male	.75	[.38, 1.48]	.63	[.28, 1.43]
Percent Black	1.04	[.88, 1.24]	1.11	[.91, 1.35]
Percent married	1.10	[.98, 1.23]	1.02	[.88, 1.19]
Percent divorced	1.18	[.89, 1.56]	1.07	[.76, 1.51]
Percent veteran	**.69***	[.55, .87]	**.64***	[.48, .84]
Percent living in MSA	1.00	[.98, 1.03]	.99	[.97, 1.02]
Ethanol consumption per capita	1.05	[.39, 2.87]	.86	[.26, 2.81]
Religious adherence	1.01	[.97, 1.05]	.99	[.94, 1.04]
Percent completed high school	1.11	[.98, 1.25]	**1.17***	[1.02, 1.34]
Drug overdose rate	1.00	[.97, 1.03]	.98	[.94, 1.02]
Percent aged 15–24	.92	[.73, 1.15]	.88	[.70, 1.10]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*p = .05.

TABLE A6 Estimates for incident rate ratios for domestic-linked mass shooting (>3 victims), using year fixed effects

Variable	Domestic-Linked Fatal Mass Shooting Incidents (n = 182 shootings) IRR	95% CI	Fatalities in Domestic-Linked Mass Shootings (n = 842 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.64	[.26, 1.59]	.62	[.24, 1.65]
Shall issue w/ discretion	.90	[.35, 2.31]	.76	[.27, 2.09]
Strict shall issue	.85	[.31, 2.38]	.70	[.23, 2.11]
Permitless	1.92	[.30, 12.36]	1.06	[.12, 9.36]
Purchaser licensing[a]	.84	[.33, 2.16]	1.46	[.57, 3.71]
Comprehensive background checks—point of sale	1.89	[.86, 4.14]	**2.25***	[1.02, 4.96]
DVRO prohibition—final orders, dating partner excluded	.94	[.34, 2.57]	.83	[.28, 2.49]
DVRO prohibition ex parte included	1.65	[.87, 3.16]	1.70	[.81, 3.57]
DVRO includes dating partners	.88	[.54, 1.45]	.83	[.50, 1.39]
DVRO surrender required	.84	[.41, 1.75]	.75	[.33, 1.70]
Violent misdemeanor prohibition	1.90	[.47, 7.77]	1.92	[.52, 7.06]
Federal assault weapons/LCM ban (gradual)	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]
State assault weapons ban (gradual)	.39	[.11, 1.34]	.30	[.09, 1.02]
Large-capacity magazine ban (gradual)	**.39***	[.20, .76]	**.26***	[.11, .60]
Gun ownership	.96	[.89, 1.03]	.95	[.88, 1.02]
Unemployment	1.04	[.82, 1.31]	1.08	[.82, 1.41]
Percent in poverty	1.03	[.91, 1.18]	1.03	[.89, 1.18]
Percent male	1.04	[.29, 3.78]	1.05	[.22, 4.98]
Percent Black	1.00	[.78, 1.29]	1.03	[.78, 1.36]
Percent married	1.02	[.79, 1.30]	1.07	[.82, 1.40]
Percent divorced	1.10	[.65, 1.84]	1.18	[.69, 2.03]
Percent veteran	.97	[.63, 1.49]	1.04	[.64, 1.71]
Percent living in MSA	1.00	[.95, 1.06]	.98	[.93, 1.04]
Ethanol consumption per capita	.64	[.10, 4.05]	.59	[.08, 4.35]
Religious adherence	1.00	[.92, 1.07]	.98	[.90, 1.06]
Percent completed high school	.99	[.81, 1.22]	.94	[.75, 1.16]
Drug overdose rate	.97	[.92, 1.04]	.97	[.91, 1.03]
Percent aged 15–24	1.13	[.81, 1.56]	1.16	[.82, 1.63]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
*p = .05.

TABLE A7 Estimates for incident rate ratios for non–domestic-linked mass shooting (>3 victims), using year fixed effects

Variable	Non–Domestic-Linked Fatal Mass Shooting incidents (*n* = 182 shootings) IRR	95% CI	Fatalities in Non–Domestic-Linked Mass Shootings (*n* = 2,057 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.92	[.46, 1.84]	1.40	[.70, 2.78]
Shall issue w/ discretion	.75	[.32, 1.74]	.98	[.38, 2.52]
Strict shall issue	1.58	[.86, 2.91]	1.68	[.82, 3.45]
Permitless	.66	[.27, 1.62]	.85	[.23, 3.13]
Purchaser licensing[a]	**.37**[*]	[.21, .67]	**.35**[*]	[.19, .65]
Comprehensive background checks—point of sale	.75	[.43, 1.31]	.83	[.38, 1.83]
DVRO prohibition—final orders, dating partner excluded	.92	[.34, 2.49]	.80	[.25, 2.52]
DVRO prohibition ex parte included	1.19	[.64, 2.22]	1.43	[.72, 2.84]
DVRO includes dating partners	.89	[.43, 1.84]	.91	[.37, 2.27]
DVRO surrender required	.66	[.34, 1.30]	.64	[.29, 1.44]
Violent misdemeanor prohibition	1.30	[.62, 2.72]	.93	[.44, 1.97]
Federal assault weapons/LCM ban (gradual)	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]
State assault weapons ban (gradual)	.62	[.24, 1.61]	.81	[.21, 3.13]
Large-capacity magazine ban (gradual)	.74	[.28, 1.97]	.58	[.15, 2.32]
Gun ownership	.98	[.94, 1.03]	.97	[.92, 1.03]
Unemployment	1.12	[.99, 1.27]	1.11	[.96, 1.28]
Percent in poverty	.99	[.91, 1.08]	.96	[.88, 1.06]
Percent male	.66	[.31, 1.41]	**.40**[*]	[.17, .95]
Percent Black	1.04	[.84, 1.29]	1.15	[.88, 1.50]
Percent married	**1.22**[*]	[1.00, 1.48]	1.08	[.86, 1.36]
Percent divorced	1.26	[.86, 1.87]	1.01	[.64, 1.58]
Percent veteran	**.58**[*]	[.43, .79]	**.52**[*]	[.35, .76]
Percent living in MSA	1.01	[.98, 1.05]	1.01	[.97, 1.05]
Ethanol consumption per capita	1.09	[.26, 4.47]	.98	[.19, 5.03]
Religious adherence	1.02	[.96, 1.08]	1.00	[.92, 1.08]
Percent completed high school	1.16	[.98, 1.36]	**1.27**[*]	[1.05, 1.53]
Drug overdose rate	1.02	[.98, 1.06]	1.00	[.96, 1.05]
Percent aged 15–24	.88	[.59, 1.33]	.76	[.48, 1.21]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

[*]p = .05.

Estimates Using Poisson Fixed-Effects Regression.

TABLE A8 Estimates for incident rate ratios for all fatal mass shootings (>3 victims), using fixed-effects poisson regression

Variable	All Fatal Mass Shooting Incidents (n = 604 shootings) IRR	95% CI	Fatalities in All Fatal Mass Shootings (n = 2, 976 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.79	[.49, 1.28]	1.07	[.61, 1.85]
Shall issue w/ discretion	.81	[.46, 1.40]	.90	[.47, 1.75]
Strict shall issue	1.11	[.67, 1.83]	1.06	[.61, 1.83]
Permitless	1.22	[.53, 2.76]	.97	[.39, 2.39]
Purchaser licensing[a]	**.49***	[.30, .82]	.61	[.37, 1.01]
Comprehensive background checks—point of sale	1.11	[.79, 1.55]	1.83	[.68, 4.87]
DVRO prohibition—final orders, dating partner excluded	.93	[.44, 1.97]	.79	[.33, 1.88]
DVRO prohibition ex parte included	1.00	[.72, 1.38]	.84	[.57, 1.24]
DVRO includes dating partners	.86	[.58, 1.28]	.85	[.55, 1.32]
DVRO surrender required	.76	[.52, 1.11]	.88	[.53, 1.46]
Violent misdemeanor prohibition	1.42	[.78, 2.59]	.97	[.45, 2.07]
Federal assault weapons/LCM ban (gradual)	.92	[.70, 1.20]	.91	[.67, 1.24]
State assault weapons ban (gradual)	.74	[.45, 1.24]	.93	[.57, 1.52]
Large-capacity magazine ban (gradual)	**.48***	[.28, .82]	**.32***	[.17, .58]
Gun ownership	.99	[.96, 1.02]	.98	[.95, 1.01]
Unemployment	1.04	[.98, 1.10]	1.03	[.95, 1.11]
Percent in poverty	1.00	[.94, 1.05]	.98	[.93, 1.04]
Percent male	.62	[.29, 1.31]	**.43***	[.19, .94]
Percent Black	1.03	[.88, 1.21]	1.12	[.88, 1.43]
Percent married	1.04	[.95, 1.14]	1.01	[.93, 1.10]
Percent divorced	1.01	[.80, 1.28]	1.01	[.76, 1.33]
Percent veteran	**.84***	[.74, .96]	.95	[.80, 1.13]
Percent living in MSA	1.00	[.98, 1.03]	.99	[.97, 1.02]
Ethanol consumption per capita	1.37	[.49, 3.81]	1.06	[.33, 3.37]
Religious adherence	1.02	[.98, 1.07]	1.00	[.94, 1.06]
Percent completed high school	1.06	[.98, 1.13]	1.07	[.99, 1.16]
Drug overdose rate	1.02	[.99, 1.05]	1.01	[.98, 1.04]
Percent aged 15–24	.86	[.70, 1.05]	.95	[.76, 1.18]
Linear time trend	.96	[.84, 1.09]	.96	[.84, 1.10]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
*p = .05.

TABLE A9 Estimates for incident rate ratios for domestic-linked mass shooting (>3 victims), using fixed-effects poisson regression

Variable	Domestic-Linked Fatal Mass Shooting incidents (*n* = 182 shootings) IRR	95% CI	Fatalities in Domestic-Linked Mass Shootings (*n* = 842 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.64	[.26, 1.58]	.73	[.29, 1.83]
Shall issue w/ discretion	1.00	[.43, 2.32]	.85	[.37, 1.95]
Strict shall issue	.98	[.38, 2.49]	.93	[.34, 2.52]
Permitless	2.94	[.51, 16.83]	2.56	[.42, 15.60]
Purchaser licensing[a]	.95	[.40, 2.22]	1.90	[.72, 4.98]
Comprehensive background checks—point of sale	1.79	[.90, 3.58]	**1.92**[*]	[1.05, 3.53]
DVRO prohibition—final orders, dating partner excluded	1.01	[.35, 2.89]	.87	[.29, 2.64]
DVRO prohibition ex parte included	1.59	[.88, 2.85]	1.51	[.81, 2.81]
DVRO includes dating partners	.90	[.57, 1.43]	.80	[.50, 1.28]
DVRO surrender required	.86	[.46, 1.61]	.84	[.45, 1.56]
Violent misdemeanor prohibition	1.60	[.44, 5.79]	1.66	[.55, 5.05]
Federal assault weapons/LCM ban (gradual)	.87	[.50, 1.50]	.89	[.51, 1.53]
State assault weapons ban (gradual)	.53	[.23, 1.20]	.68	[.32, 1.43]
Large-capacity magazine ban (gradual)	**.38**[*]	[.21, .70]	**.27**[*]	[.12, .59]
Gun ownership	.98	[.91, 1.05]	.97	[.91, 1.04]
Unemployment	1.04	[.91, 1.19]	1.09	[.94, 1.25]
Percent in poverty	1.00	[.88, 1.14]	.99	[.88, 1.12]
Percent male	.87	[.26, 2.89]	.75	[.21, 2.66]
Percent Black	1.02	[.82, 1.27]	1.06	[.85, 1.33]
Percent married	.96	[.83, 1.12]	.96	[.83, 1.11]
Percent divorced	.90	[.64, 1.27]	.95	[.68, 1.34]
Percent veteran	.99	[.82, 1.20]	1.03	[.85, 1.27]
Percent living in MSA	1.00	[.95, 1.06]	.99	[.94, 1.04]
Ethanol consumption per capita	1.10	[.16, 7.46]	1.07	[.13, 8.41]
Religious adherence	1.03	[.94, 1.12]	1.01	[.92, 1.11]
Percent completed high school	1.02	[.92, 1.14]	1.01	[.91, 1.13]
Drug overdose rate	.99	[.93, 1.05]	.98	[.92, 1.04]
Percent aged 15–24	1.07	[.79, 1.47]	1.17	[.83, 1.64]
Linear time trend	1.01	[.80, 1.27]	1.04	[.83, 1.30]
Quadratic time trend	1.00	[.99, 1.01]	1.00	[.99, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
[*]$p = .05$.

TABLE A10 Estimates for incident rate ratios for non–domestic-linked mass shooting (>3 victims), using fixed-effects poisson regression

Variable	Non–Domestic-Linked Fatal Mass Shooting incidents (*n* = 182 shootings) IRR	95% CI	Fatalities in Non–Domestic-Linked Mass Shootings (*n* = 2,057 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	.88	[.46, 1.70]	1.21	[.62, 2.36]
Shall issue w/ discretion	.76	[.34, 1.71]	.92	[.38, 2.22]
Strict shall issue	1.28	[.76, 2.18]	1.20	[.66, 2.15]
Permitless	.58	[.24, 1.42]	.75	[.19, 2.92]
Purchaser licensing[a]	**.42***	[.22, .80]	**.45***	[.25, .83]
Comprehensive background checks—point of sale	.87	[.50, 1.51]	1.84	[.49, 6.87]
DVRO prohibition—final orders, dating partner excluded	.91	[.35, 2.38]	.75	[.25, 2.27]
DVRO prohibition ex parte included	.83	[.46, 1.50]	.68	[.38, 1.22]
DVRO includes dating partners	.84	[.46, 1.53]	.85	[.45, 1.62]
DVRO surrender required	.76	[.39, 1.49]	.99	[.45, 2.20]
Violent misdemeanor prohibition	1.22	[.60, 2.50]	.69	[.28, 1.72]
Federal assault weapons/LCM ban (gradual)	.96	[.65, 1.41]	.95	[.62, 1.45]
State assault weapons ban (gradual)	.79	[.42, 1.48]	.94	[.50, 1.76]
Large-capacity magazine ban (gradual)	.56	[.26, 1.19]	**.35***	[.16, .76]
Gun ownership	1.01	[.97, 1.04]	.99	[.96, 1.03]
Unemployment	1.04	[.97, 1.11]	1.01	[.92, 1.11]
Percent in poverty	1.00	[.93, 1.07]	.98	[.92, 1.05]
Percent male	.52	[.19, 1.38]	**.40***	[.16, 1.00]
Percent Black	1.02	[.83, 1.25]	1.13	[.81, 1.58]
Percent married	1.08	[.95, 1.23]	1.03	[.90, 1.18]
Percent divorced	1.10	[.79, 1.53]	.99	[.67, 1.46]
Percent veteran	**.77***	[.64, .94]	.95	[.75, 1.18]
Percent living in MSA	1.01	[.98, 1.05]	1.01	[.97, 1.05]
Ethanol consumption per capita	1.32	[.30, 5.94]	1.00	[.21, 4.87]
Religious adherence	1.01	[.96, 1.08]	.99	[.92, 1.07]
Percent completed high school	1.05	[.94, 1.18]	1.09	[.97, 1.22]
Drug overdose rate	**1.04***	[1.01, 1.08]	1.01	[.98, 1.05]
Percent aged 15–24	.78	[.58, 1.04]	.85	[.61, 1.17]
Linear time trend	.94	[.81, 1.09]	.94	[.80, 1.10]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*$p = .05$.

Estimates Omitting Major Mass Shooting Incidents From 2012 in Colorado (Aurora) and Connecticut (Newtown).

TABLE A11 Estimates for incident rate ratios for all fatal mass shootings (>3 victims), Omitting Newtown and Aurora shootings

	All Fatal Mass Shooting Incidents ($n = 602$ shootings)		Fatalities in All Fatal Mass Shootings ($n = 2,937$ fatalities)	
Variable	**IRR**	**95% CI**	**IRR**	**95% CI**
Concealed carry permit—may issue reference No issue	.93	[.55, 1.57]	1.50	[.81, 2.75]
Shall issue w/ discretion	.89	[.50, 1.60]	1.10	[.54, 2.24]
Strict shall issue	1.30	[.73, 2.30]	1.52	[.76, 3.06]
Permitless	1.31	[.51, 3.34]	1.09	[.34, 3.50]
Purchaser licensing[a]	**.40**[*]	[.23, .69]	**.33**[*]	[.19, .59]
Comprehensive background checks—point of sale	1.11	[.78, 1.59]	1.41	[.73, 2.74]
DVRO prohibition—final orders, dating partner excluded	.89	[.43, 1.85]	.77	[.34, 1.77]
DVRO prohibition ex parte included	1.13	[.77, 1.64]	1.21	[.75, 1.94]
DVRO includes dating partners	.90	[.57, 1.45]	.93	[.51, 1.70]
DVRO surrender required	.76	[.49, 1.17]	.76	[.45, 1.30]
Violent misdemeanor prohibition	1.51	[.78, 2.91]	1.27	[.63, 2.59]
Federal assault weapons/LCM ban (gradual)	.92	[.68, 1.26]	.96	[.63, 1.44]
State assault weapons ban (gradual)	.67	[.33, 1.38]	.90	[.30, 2.74]
Large-capacity magazine ban (gradual)	.56	[.30, 1.03]	.40	[.14, 1.14]
Gun ownership	.98	[.95, 1.02]	.96	[.93, 1.00]
Unemployment	1.02	[.95, 1.10]	1.01	[.91, 1.11]
Percent in poverty	1.01	[.95, 1.07]	1.00	[.93, 1.07]
Percent male	.82	[.39, 1.75]	.90	[.39, 2.08]
Percent Black	1.07	[.91, 1.25]	1.17	[.96, 1.43]
Percent married	1.03	[.94, 1.13]	.99	[.89, 1.11]
Percent divorced	1.02	[.79, 1.31]	.96	[.72, 1.28]
Percent veteran	**.86**[*]	[.75, .98]	.91	[.78, 1.07]
Percent living in MSA	1.01	[.98, 1.03]	1.01	[.98, 1.03]
Ethanol consumption per capita	1.08	[.39, 2.97]	.79	[.23, 2.66]
Religious adherence	1.01	[.97, 1.06]	.99	[.94, 1.05]
Percent completed high school	1.06	[.98, 1.14]	1.07	[.99, 1.17]
Drug overdose rate	1.01	[.97, 1.05]	.99	[.95, 1.03]
Percent aged 15–24	.83	[.68, 1.02]	.86	[.69, 1.08]
Linear time trend	.92	[.81, 1.05]	.89	[.77, 1.03]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.00]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
[*]$p = .05$.

TABLE A12 Estimates for incident rate ratios for domestic-linked mass shooting (>3 victims), Omitting Newtown and Aurora shootings

Variable	Domestic-Linked Fatal Mass Shooting Incidents (n = 181 shootings) Law Variables + Covariates IRR	95% CI	Fatalities in Domestic-Linked Mass Shootings (n = 815 fatalities) Law Variables + Covariates IRR	95% CI
Concealed carry permit—may issue reference No issue	.67	[.26, 1.70]	.75	[.28, 2.02]
Shall issue w/ discretion	.99	[.42, 2.35]	.84	[.34, 2.04]
Strict shall issue	.97	[.36, 2.66]	.93	[.30, 2.86]
Permitless	2.49	[.37, 16.69]	1.72	[.19, 15.52]
Purchaser licensing[a]	.60	[.16, 2 .20]	.60	[.14, 2.53]
Comprehensive background checks—point of sale	1.90	[.91, 4.00]	**2.17**[*]	[1.05, 4.48]
DVRO prohibition—final orders, dating partner excluded	.91	[.32, 2.60]	.71	[.23, 2.20]
DVRO prohibition ex parte included	1.60	[.89, 2.87]	1.66	[.87, 3.17]
DVRO includes dating partners	.92	[.58, 1.47]	.83	[.51, 1.36]
DVRO surrender required	.84	[.44, 1.62]	.78	[.38, 1.62]
Violent misdemeanor prohibition	1.76	[.42, 7.41]	1.81	[.51, 6.47]
Federal assault weapons/LCM ban (gradual)	.87	[.50, 1.52]	.85	[.46, 1.57]
State assault weapons ban (gradual)	.34	[.10, 1.14]	**.24**[*]	[.06, .90]
Large-capacity magazine ban (gradual)	**.46**[*]	[.23, .89]	**.45**[*]	[.22, .91]
Gun ownership	.97	[.90, 1.05]	.97	[.90, 1.05]
Unemployment	1.05	[.90, 1.21]	1.08	[.91, 1.28]
Percent in poverty	1.01	[.88, 1.15]	1.00	[.87, 1.14]
Percent male	1.09	[.31, 3.90]	1.27	[.29, 5.52]
Percent Black	1.00	[.80, 1.25]	1.01	[.80, 1.27]
Percent married	.96	[.82, 1.13]	.97	[.81, 1.16]
Percent divorced	.86	[.59, 1.27]	.82	[.52, 1.27]
Percent veteran	1.00	[.83, 1.21]	1.06	[.87, 1.30]
Percent living in MSA	1.00	[.95, 1.06]	.99	[.94, 1.05]
Ethanol consumption per capita	.93	[.14, 6.29]	.83	[.11, 6.07]
Religious adherence	1.02	[.94, 1.11]	1.01	[.94, 1.10]
Percent completed high school	1.02	[.91, 1.15]	1.01	[.89, 1.13]
Drug overdose rate	.98	[.92, 1.04]	.98	[.91, 1.05]
Percent aged 15–24	1.00	[.75, 1.33]	.99	[.75, 1.30]
Linear time trend	.98	[.79, 1.23]	1.02	[.81, 1.28]
Quadratic time trend	1.00	[.99, 1.01]	1.00	[1.00, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
[*]p = .05.

TABLE A13 Estimates for incident rate ratios for non–domestic-linked mass shooting (>3 victims), Omitting Newtown and Aurora shootings

Variable	Non–Domestic-Linked Fatal Mass Shooting incidents (*n* = 181 shootings) IRR	95% CI	Fatalities in Non–Domestic-Linked Mass Shootings (*n* = 2,045 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	1.00	[.49, 2.03]	1.72	[.79, 3.75]
Shall issue w/ discretion	.81	[.36, 1.82]	1.06	[.42, 2.68]
Strict shall issue	1.51	[.85, 2.69]	1.79	[.86, 3.72]
Permitless	.67	[.25, 1.78]	1.08	[.24, 4.76]
Purchaser licensing[a]	**.38***	[.20, .70]	**.34***	[.18, .62]
Comprehensive background checks—point of sale	.85	[.48, 1.51]	1.11	[.45, 2.74]
DVRO prohibition—final orders, dating partner excluded	.90	[.33, 2.52]	.75	[.25, 2.22]
DVRO prohibition ex parte included	1.04	[.54, 2.01]	1.20	[.60, 2.39]
DVRO includes dating partners	.90	[.45, 1.81]	.98	[.43, 2.26]
DVRO surrender required	.75	[.35, 1.61]	.84	[.35, 2.00]
Violent misdemeanor prohibition	1.33	[.65, 2.74]	.99	[.48, 2.06]
Federal assault weapons/LCM ban (gradual)	.98	[.65, 1.47]	1.09	[.66, 1.80]
State assault weapons ban (gradual)	.72	[.31, 1.69]	.94	[.24, 3.75]
Large-capacity magazine ban (gradual)	.67	[.27, 1.69]	.47	[.12, 1.94]
Gun ownership	1.00	[.96, 1.04]	.97	[.92, 1.02]
Unemployment	1.03	[.96, 1.11]	1.01	[.92, 1.11]
Percent in poverty	1.00	[.94, 1.07]	.98	[.91, 1.07]
Percent male	.68	[.27, 1.73]	.69	[.25, 1.93]
Percent Black	1.08	[.87, 1.33]	1.27	[.94, 1.72]
Percent married	1.06	[.92, 1.21]	.98	[.84, 1.14]
Percent divorced	1.10	[.77, 1.57]	.94	[.64, 1.37]
Percent veteran	**.79***	[.65, .96]	.88	[.69, 1.11]
Percent living in MSA	1.01	[.98, 1.05]	1.02	[.97, 1.06]
Ethanol consumption per capita	1.13	[.24, 5.21]	.86	[.13, 5.51]
Religious adherence	1.01	[.95, 1.08]	.99	[.91, 1.07]
Percent completed high school	1.06	[.95, 1.19]	1.11	[.97, 1.26]
Drug overdose rate	1.04	[1.00, 1.08]	1.01	[.96, 1.06]
Percent aged 15–24	.78	[.57, 1.07]	.80	[.54, 1.18]
Linear time trend	.91	[.77, 1.07]	.86	[.72, 1.04]
Quadratic time trend	1.00	[1.00, 1.00]	1.00	[1.00, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*$p = .05$.

Estimates Using Different Definitions of "Mass Shooting"—Shootings With Fatalities > 4 and Shootings With Fatalities > 5.

TABLE A14 Estimates for incident rate ratios for all mass shooting (>4 victims)

Variable	All Fatal Mass Shooting Incidents (n = 198 shootings) IRR	95% CI	Fatalities in All Fatal Mass Shootings (n = 1, 352 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	**4.14***	[1.57, 1.87]	**8.41***	[3.00, 23.57]
Shall issue w/ discretion	.96	[.31, 2.94]	1.23	[.35, 4.30]
Strict shall issue	2.24	[.91, 5.49]	2.60	[.99, 6.78]
Permitless	.91	[.14, 5.78]	1.53	[.19, 12.43]
Purchaser licensing[a]	.52	[.15, 1.83]	.44	[.09, 2.18]
Comprehensive background checks—point of sale	1.94	[.85, 4.41]	3.65	[.74, 18.05]
DVRO prohibition—final orders, dating partner excluded	.70	[.22, 2.21]	.63	[.15, 2.61]
DVRO prohibition ex parte included	.97	[.54, 1.73]	1.11	[.55, 2.26]
DVRO includes dating partners	.58	[.30, 1.13]	.61	[.24, 1.52]
DVRO surrender required	.75	[.40, 1.42]	.79	[.32, 1.95]
Violent misdemeanor prohibition	2.10	[.55, 8.02]	1.34	[.35, 5.05]
Federal assault weapons/LCM ban (gradual)	1.00	[.50, 2.02]	.92	[.42, 2.01]
State assault weapons ban (gradual)	.58	[.13, 2.62]	1.41	[.09, 2.94]
Large-capacity magazine ban (gradual)	**.20***	[.06, .65]	**.08***	[.01, .92]
Gun ownership	.97	[.91, 1.02]	.94	[.88, 1.00]
Unemployment	1.08	[.97, 1.21]	1.08	[.95, 1.24]
Percent in poverty	.95	[.85, 1.06]	.93	[.81, 1.06]
Percent male	.43	[.12, 1.59]	.39	[.08, 1.94]
Percent Black	.92	[.66, 1.28]	1.05	[.68, 1.61]
Percent married	.90	[.80, 1.01]	.88	[.75, 1.04]
Percent divorced	.81	[.55, 1.19]	.83	[.53, 1.29]
Percent veteran	.88	[.69, 1.12]	.94	[.70, 1.26]
Percent living in MSA	.98	[.94, 1.02]	.97	[.92, 1.02]
Ethanol consumption per capita	.86	[.13, 5.73]	.90	[.09, 9.22]
Religious adherence	.93	[.86, 1.00]	**.90***	[.82, 1.00]
Percent completed high school	**1.17***	[1.05, 1.30]	**1.19***	[1.05, 1.34]
Drug overdose rate	1.02	[.96, 1.07]	.99	[.94, 1.04]
Percent aged 15–24	1.14	[.84, 1.55]	1.13	[.77, 1.65]
Linear time trend	.96	[.77, 1.20]	.93	[.73, 1.19]
Quadratic time trend	1.00	[.99, 1.00]	1.00	[1.00, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.
$^*p$ = .05.

TABLE A15 Estimates for incident rate ratios for all mass shooting (>5 victims)

Variable	All Fatal Mass Shooting Incidents (>5 victims) (*n* = 92 shootings) IRR	95% CI	Fatalities in All Fatal Mass Shootings (*n* = 822 fatalities) IRR	95% CI
Concealed carry permit—may issue reference No issue	**1.77***	[1.99, 58.31]	**25.74***	[4.03, 164.2]
Shall issue w/ discretion	2.13	[.27, 16.58]	1.95	[.17, 21.93]
Strict shall issue	1.93	[.30, 12.41]	1.79	[.22, 14.29]
Permitless	3.81	[.34, 42.94]	2.99	[.22, 41.29]
Purchaser licensing[a]	.87	[.32, 2.33]	.69	[.24, 2.05]
Comprehensive background checks—point of sale	2.27	[.52, 9.84]	6.98	[.82, 59.36]
DVRO prohibition—final orders, dating partner excluded	.61	[.11, 3.35]	.36	[.05, 2.62]
DVRO prohibition ex parte included	1.16	[.48, 2.79]	1.07	[.41, 2.83]
DVRO includes dating partners	.98	[.27, 3.58]	.94	[.21, 4.24]
DVRO surrender required	.51	[.15, 1.76]	.88	[.19, 4.02]
Violent misdemeanor prohibition	.72	[.16, 3.26]	.27	[.04, 1.65]
Federal assault weapons/LCM ban (gradual)	.77	[.31, 1.96]	.69	[.21, 2.22]
State assault weapons ban (gradual)	1.04	[.17, 6.36]	1.38	[.12, 15.48]
Large-capacity magazine ban (gradual)	**.14***	[.03, .70]	**.05***	[.00, .51]
Gun ownership	.96	[.89, 1.04]	.92	[.84, 1.01]
Unemployment	1.16	[.98, 1.37]	1.17	[.95, 1.45]
Percent in poverty	.93	[.80, 1.10]	.88	[.72, 1.07]
Percent male	.26	[.03, 2.14]	.42	[.04, 4.62]
Percent Black	.82	[.52, 1.30]	.91	[.53, 1.57]
Percent married	1.05	[.86, 1.28]	1.03	[.79, 1.33]
Percent divorced	1.03	[.56, 1.91]	1.06	[.54, 2.08]
Percent veteran	.86	[.64, 1.18]	.92	[.63, 1.34]
Percent living in MSA	.96	[.88, 1.05]	.94	[.84, 1.04]
Ethanol consumption per capita	5.43	[.23, 126.96]	1.79	[.04, 77.79]
Religious adherence	.91	[.80, 1.03]	.88	[.75, 1.03]
Percent completed high school	1.16	[.97, 1.39]	1.19	[.97, 1.47]
Drug overdose rate	.98	[.89, 1.08]	.95	[.86, 1.05]
Percent aged 15–24	1.16	[.66, 2.04]	1.20	[.59, 2.45]
Linear time trend	1.10	[.83, 1.44]	.99	[.74, 1.33]
Quadratic time trend	1.00	[.99, 1.01]	1.00	[.99, 1.01]

[a]Handgun purchaser licensing with in-person application and/or fingerprinting of applicant.

*$p = .05$.

Declaration Exhibit 3

Purchaser Licensing, Point-of-Sale Background Check Laws, and Firearm Homicide and Suicide in 4 US States, 1985–2017

Alexander D. McCourt, JD, PhD, MPH, Cassandra K. Crifasi, PhD, MPH, Elizabeth A. Stuart, PhD, Jon S. Vernick, JD, MPH, Rose M. C. Kagawa, PhD, MPH, Garen J. Wintemute, MD, MPH, and Daniel W. Webster, ScD, MPH

Objectives. To estimate and compare the effects of state background check policies on firearm-related mortality in 4 US states.

Methods. Annual data from 1985 to 2017 were used to examine Maryland and Pennsylvania, which implemented point-of-sale comprehensive background check (CBC) laws for handgun purchasers; Connecticut, which adopted a handgun purchaser licensing law; and Missouri, which repealed a similar law. Using synthetic control methods, we estimated the effects of these laws on homicide and suicide rates stratified by firearm involvement.

Results. There was no consistent relationship between CBC laws and mortality rates. There were estimated decreases in firearm homicide (27.8%) and firearm suicide (23.2%–40.5%) rates associated with Connecticut's law. There were estimated increases in firearm homicide (47.3%), nonfirearm homicide (18.1%), and firearm suicide (23.5%) rates associated with Missouri's repeal.

Conclusions. Purchaser licensing laws coupled with CBC requirements were consistently associated with lower firearm homicide and suicide rates, but CBC laws alone were not.

Public Health Implications. Our results contribute to a body of research showing that CBC laws are not associated with reductions in firearm-related deaths unless they are coupled with handgun purchaser licensing laws. (*Am J Public Health.* 2020;110:1546–1552. doi:10.2105/AJPH.2020.305822)

Firearms were the second-leading mechanism of death by injury in the United States in 2018, resulting in 39 740 deaths.[1] Laws intended to keep firearms from individuals at the highest risk of harming themselves or others may reduce firearm-related deaths, but they rely on background checks and other systems for vetting those seeking to acquire firearms.

Although federal law requires individuals who purchase firearms from federally licensed dealers to pass a background check, no background check is required for purchases from private sellers. As of January 2020, 21 states required a background check for at least some private firearm sales. These state laws can be sorted into 2 broad categories: point-of-sale comprehensive background check (CBC) laws and purchaser licensing laws. Both categories require firearm purchasers to pass a background check prior to a sale or transfer, but they differ with respect to timing and process.

CBC laws require a background check for private purchasers at the point of sale. Prospective purchasers and sellers typically go to federally licensed dealers who process the transfer by submitting applications to the Federal Bureau of Investigation or state law enforcement agencies to determine whether the applicant is legally qualified to acquire a firearm. Under purchaser licensing laws, a prospective purchaser is required to apply for a license directly to a state or local law enforcement agency that vets the application and initiates a background check, often aided by mandated fingerprinting. Private sellers and federally licensed dealers can sell handguns only to individuals with valid licenses. Absent a CBC law, residents of states with a licensing law may not need to undergo a point-of-sale background check if they have a valid license to purchase. In some states, a valid permit to carry a concealed handgun can substitute for a license to purchase or a point-of-sale background check.

Although individual-level studies of background checks suggest that they are effective,[2–4] recent state-level research casts doubt on the population-level effectiveness of CBC laws alone in reducing firearm-related deaths.[5–7] Studies suggesting CBC law effectiveness have methodological limitations including cross-sectional designs[8] and exclusion of CBC laws that apply only to handguns.[9] In 2018, handguns accounted for 90% of the firearms used in homicides in which the type of firearm was specified.[10]

Studies in several US states have shown that firearm purchaser licensing laws are

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associated with reductions in firearm homicides.[3,8] Connecticut enacted a handgun purchaser licensing law in 1995 that was associated with significant decreases in rates of firearm homicides[11] and firearm suicides.[12] After the 2007 repeal of Missouri's handgun purchaser licensing law that also functioned as a point-of-sale CBC law, rates of firearm homicides[13,14] and suicides[12] increased in the state, as did indicators of guns diverted for criminal use.[15] Critics of these studies identified the relatively short periods of postlaw data in Missouri and Connecticut and possible overreliance on Rhode Island as a point of comparison with Connecticut's trends.[16]

In this study, we improved on prior analyses of purchaser licensing laws in Connecticut and Missouri and applied similar methods to analyze point-of-sale-only laws in Maryland and Pennsylvania, which adopted typical CBC laws in 1996 and 1995, respectively. We lengthened the period of observation for Connecticut and Missouri and applied a uniform analytic approach across all 4 states, comparing the findings with respect to CBC and licensing policies.

METHODS

Following the example of some earlier studies of licensing and CBC laws,[6,7,11,13] we used the synthetic control method[17] to compare each state's homicide and suicide rates with estimates of the counterfactual: each intervention state's forecasted homicide and suicide rates had the law not been enacted. In accord with the synthetic control method, we used a series of preintervention outcomes and other covariates to construct a convex combination of weighted donor states that best approximated the pretreatment outcome and covariate trends in the treated state (the state with the relevant policy change). The weights were determined on the basis of their capability to minimize the prediction error during the period prior to the law change being evaluated. The donor pool of potential controls contained states that did not have the law of interest in place during the study period. This weighted combination of donor states—the synthetic control—was compared with the treated state in the posttreatment period to estimate the effect of the intervention. We present the mean square predicted error (MSPE) for the preintervention period as a measure of model fit.

Each state law change was evaluated for its association with rates of firearm homicides, nonfirearm homicides, firearm suicides, and nonfirearm suicides. Each prelaw period was 10 years; the postlaw period was determined by the amount of postlaw data available after the law change and the legal environment of each state. The time period for Pennsylvania's 1995 CBC law ran from 1985 to 2017. For Maryland's 1996 CBC law, the postlaw period was truncated at 2013 because the state adopted a handgun purchaser licensing law late that year. The study period for Missouri's repeal of its 2007 licensing law started in 1997 and ended in 2016 because Missouri began allowing permitless concealed carry on January 1, 2017. Prior work has shown an association between less restrictive concealed carry laws and violent crime.[18] For Connecticut, we present data through 2017 but also provide estimates that exclude 2013 to 2017 because of a state program under which several cities began implementing focused deterrence programs to curb gang violence.[19]

The donor pools of potential controls for Pennsylvania (29 states), Maryland (33 states), and Connecticut (39 states) consisted of states that did not have the law of interest in place throughout the study periods just described. Missouri's donor pool (8 states) consisted of states that had a purchaser licensing law for the entirety of the study period.

For each model, the effect was estimated by determining the difference in postlaw means between the treated state and the synthetic control and calculating the percentage increase or decrease from the synthetic control. To assess whether the estimated effects of CBC and purchaser licensing laws were unusual with respect to effects that would be estimated in other states, we performed placebo tests with all states in the donor pool for each law change.[17] The estimated effect for the treated state was compared with the placebo effect distribution estimated from the donor states. To make a reliable inference, we had to find that only a small proportion of control states had a more extreme placebo effect estimate than the effect estimated for the true treated state. We used this proportion as a permutation distribution pseudo *P* value. Because a synthetic control that adequately fit the preintervention data could not be estimated for each donor state, we restricted the placebo tests to the subset of donor states with prelaw MSPEs less than 5 times the treated state's prelaw MSPE to avoid comparisons with synthetic controls that had poor fits.

We used death certificate data obtained from the National Center for Health Statistics through the CDC WONDER database to generate homicide and suicide mortality rates.[20] Because annual state suicide data are often volatile, we smoothed suicide mortality rates by analyzing 3-year moving averages. Annual state-level predictors were chosen on the basis of prior research and theoretical relationships between sociodemographic variables and the dependent variables of interest.

For homicide, state-level predictors were population size, law enforcement expenditures per capita, law enforcement officer population, percentage of the population identifying as Black, percentage of the population identifying as Latino, the Gini coefficient (a measure of income inequality), percentage of the population 15 to 24 years of age, percentage of the population 0 to 18 years of age, percentage of the population living in a metropolitan statistical area, robbery rate, population density, poverty rate, jobs per capita, average individual income per capita, unemployment rate, and incarceration rate.

For suicide, the predictors were unemployment rate, poverty rate, percentage of the population identifying as male, percentage of the population reporting being married, percentage of the population identifying as Black, percentage of the population identifying as a veteran, percentage of the population living in a metropolitan statistical area, ethanol consumption per capita, religious adherence, educational attainment, and overdose rate.

Each model included prelaw averages for all of these predictors and values of the outcome variable for every other prelaw year. When necessary, missing predictor data from intercensal years were interpolated. These data were obtained from the Bureau of Economic Analysis,[21] the Bureau of Labor Statistics,[22] the Census Bureau,[23] and the Federal Bureau of Investigation's Uniform Crime Report.[24]

RESULTS

The synthetic control models revealed no consistent relationship between

comprehensive background check laws and firearm mortality in Maryland and Pennsylvania. There were, however, consistent relationships between firearm mortality and purchaser licensing laws in Connecticut and Missouri. Measures of synthetic control model fit, donor states contributing to each synthetic control, and donor weights are presented in Appendix Table A (available as a supplement to the online version of this article at http://www.ajph.org). The placebo results we report are the proportions of control states that had a more extreme placebo effect estimate than the effect estimated for the true treated state. We also report these proportions as fractions, with the number of states with a more extreme placebo effect estimate in the numerator and the number of total control states in the denominator. We restricted the denominator to the subset of donor states with prelaw MSPEs less than 5 times the treated state's prelaw MSPE.

Comprehensive Background Check Laws

Results for Maryland and Pennsylvania are presented in Table 1. After implementation of a CBC law (1996–2013), Maryland saw a 17.5% increase in firearm homicide rates relative to its synthetic control (placebo = 0.06; 2/32) and a 33.2% increase in nonfirearm homicide rates (placebo = 0.06; 2/33). Maryland's firearm suicide rate was 15.4% lower than that of its synthetic control following the state's passage of a CBC law (placebo = 0.13; 3/24), but there was also a 21.8% decrease in nonfirearm suicides (placebo = 0.03; 1/32) relative to the synthetic control.

Pennsylvania's firearm homicide rate was 21.5% higher than that of its synthetic control for the post–CBC law period 1996 to 2017 (placebo = 0.13; 3/23), whereas its nonfirearm homicide rate was 10.0% lower (placebo = 0.26; 5/19). During the same period, Pennsylvania saw a 5.3% increase in firearm suicides relative to its synthetic control (placebo = 0.21; 4/19) and an 11.8% decrease in nonfirearm suicides (placebo = 0.09; 1/11).

We performed post hoc analyses to determine whether these results might be partially explained by factors unique to the largest cities in Maryland and Pennsylvania, which accounted for a substantial share of homicides in the 2 states. When Baltimore data were excluded from the Maryland model, the CBC law was associated with insignificant increases in both firearm (3.1%; placebo = 0.34; 11/32) and nonfirearm (10.8%; placebo = 0.17; 4/24) homicides. However, the estimated effect of the CBC law in Pennsylvania on firearm homicides did not diminish when Philadelphia data were excluded (23.9%; placebo = 0.14; 2/14). Nonfirearm homicides increased 4.1% in the model without Philadelphia (placebo = 0.33; 5/15).

Purchaser Licensing Laws

Purchaser licensing laws were more clearly associated with changes in firearm homicide rates (Table 2 and Figure 1). After implementation of Connecticut's licensing law, there was a 27.8% decrease in firearm homicides relative to the state's synthetic control from 1995 to 2017 (placebo = 0.03; 1/38). This effect was similar when deaths from the 2012 Newtown school shooting were removed from homicide counts (Appendix Table J, available as a supplement to the online version of this article at http://www.ajph.org; change = –24.2%; placebo = 0.00; 0/35). The estimate for the effect of Connecticut's licensing law is somewhat smaller if the data extend only to 2012, before focused deterrence programs curbed urban gang violence in several of the state's cities (Appendix Table I, available as a supplement to the online version of this article at http://www.ajph.org; change = –19.9%; placebo = 0.03; 1/34). Nonfirearm homicide rates did not change relative to the synthetic control over the period from 1995 to 2017 (placebo = 0.61; 20/33).

From the 1995 implementation of its law through 2017, Connecticut saw a 32.8% decrease in firearm suicides (Table 2 and Figure 2; placebo = 0.06; 2/35) and a 3.3% decrease in nonfirearm suicides (placebo = 0.60; 15/25) relative to its synthetic control. In 1999, Connecticut adopted a law akin to an extreme risk protection order law. Under this law, police are authorized to temporarily take guns from individuals when there is probable cause to believe that they are at imminent risk of injuring themselves or

TABLE 1—Overall Synthetic Control Results for Point-of-Sale Comprehensive Background Check (CBC) Laws: Maryland and Pennsylvania, 1995 and 1996

	Firearm			Nonfirearm		
Model	MSPE	Effect, %	Placebo No./Total No. (%)[a]	MSPE	Effect, %	Placebo No./Total No. (%)[a]
Homicide						
Maryland 1996 CBC law	0.531	+17.5	2/32 (0.06)	0.406	+33.2	2/33 (0.06)
Maryland 1996 CBC law (excluding Baltimore)	0.440	+3.1	11/32 (0.34)	0.055	+10.8	4/24 (0.17)
Pennsylvania 1995 CBC law	0.167	+21.5	3/23 (0.13)	0.057	−10.0	5/19 (0.26)
Pennsylvania 1995 CBC law (excluding Philadelphia)	0.044	+23.9	2/14 (0.14)	0.027	+4.1	5/15 (0.33)
Suicide						
Maryland 1996 CBC law	0.060	−15.4	3/24 (0.125)	0.053	−21.8	1/32 (0.03)
Pennsylvania 1995 CBC law	0.024	+5.3	4/19 (0.21)	0.003	−11.8	1/11 (0.09)

Note. MSPE = mean square predicted error.

[a]The placebo results reported are the proportions of control states that had a more extreme placebo effect estimate than the effect actually estimated for the true treated state. We restricted the denominator to the subset of donor states with prelaw MSPEs less than 5 times the treated state's prelaw MSPE.

TABLE 2—Overall Synthetic Control Results for Purchaser Licensing Laws: Connecticut and Missouri, 1995 and 2007

	Firearm			Nonfirearm		
Model	MSPE	Effect, %	Placebo No./Total No. (%)[a]	MSPE	Effect, %	Placebo No./Total No. (%)[a]
Homicide						
Connecticut 1995 purchaser licensing	0.371	−27.8	1/38 (0.03)	0.089	−0.7	20/33 (0.61)
Missouri 2007 purchaser licensing repeal	0.257	+47.3	0/6 (0.00)	0.037	+18.1	0/8 (0.00)
Suicide						
Connecticut 1995 purchaser licensing (through 2017)	0.109	−32.8	2/35 (0.06)	0.008	−3.3	15/25 (0.60)
Connecticut 1995 purchaser licensing (through 2006)		−23.2			−3.2	
Connecticut 1995 purchaser licensing (2007–2017)		−40.5			−3.4	
Missouri 2007 purchaser licensing repeal	0.208	+23.5	0/7 (0.00)	0.065	+6.9	1/4 (0.25)

Note. MSPE = mean square predicted error.
[a]The placebo results reported are the proportions of control states that had a more extreme placebo effect estimate than the effect actually estimated for the true treated state. We restricted the denominator to the subset of donor states with prelaw MSPEs less than 5 times the treated state's prelaw MSPE.

others. Despite this law, very few gun removals were carried out until 2007, after the mass shooting at Virginia Tech.[25] Research has shown that individuals subjected to these orders are more often suicidal than homicidal and that the removal law is associated with decreases in firearm suicides.[26,27]

To examine the possible effects of the removal law on our models of firearm and nonfirearm suicides in Connecticut, we split the effect estimate into 2 periods: 1995 to 2006 and 2007 to 2017. From 1995 to 2006, there was a 23.2% decrease in firearm suicides and a 3.2% decrease in nonfirearm suicides in Connecticut relative to the synthetic control. From 2007 to 2017, there was a 40.5% decrease in firearm suicides and a 3.4% decrease in nonfirearm suicides.

From 2007 to 2016, following the repeal of its purchaser licensing law, Missouri's firearm homicide rate rose 47.3% relative to its synthetic control (Table 2 and Figure 1; placebo = 0.00; 0/6). Over the same period, there was an 18.1% increase in nonfirearm homicides relative to the synthetic control (placebo = 0.00; 0/8). The estimated effect on firearm homicides was 2.6 times larger than that for nonfirearm homicides. There was an abrupt increase in firearm homicides immediately after the law's repeal and no such change in nonfirearm homicides (Figure 1 and Appendix Figure F, available as a supplement to the online version of this article at http://www.ajph.org). Missouri's repeal of handgun purchaser licensing was associated with a 23.5% increase in firearm suicides (placebo = 0.00; 0/7) and a 6.9% increase in nonfirearm suicides (placebo = 0.25; 1/4) relative to the synthetic control (Table 2). Full truncated 10-year model results for Connecticut, Maryland, and Pennsylvania, as well as additional figures for all 4 states, are available in the appendix.

DISCUSSION

Across the 4 state law changes examined in this study, purchaser licensing laws were consistently associated with lower rates of both firearm homicides and firearm suicides, but point-of-sale CBC laws were not. Relative to Connecticut's synthetic control, we estimated a 27.8% overall decrease in the state's firearm homicide rate and a 32.8% overall decrease in its firearm suicide rate. The decrease in firearm suicides was somewhat greater after the 2007 implementation of a risk-based firearm removal law. Although this could indicate complementary effects of Connecticut's purchaser licensing and gun removal laws, the number of removal orders is likely too small to achieve population-wide effects. A more plausible explanation is that suicide mortality continued to decrease because of a growing effect of licensing stemming from reduced access to firearms in the state. For Missouri, we estimated a 47.3% overall increase in firearm homicides and a 23.5% increase in firearm suicides. In tandem, the estimates for Connecticut and Missouri suggest that purchaser licensing laws are protective.

Our results are consistent with prior studies that also revealed protective effects of Connecticut's and Missouri's handgun purchaser laws.[11–14] Our study provides additional years of data and new statistical models that indicate larger protective effects for suicides in both states. In comparison with previous studies, our estimates of changes in firearm homicide rates associated with purchaser licensing were larger in the case of Missouri and smaller in the case of Connecticut. Other studies designed to estimate average associations across many law changes have also shown that licensing laws are associated with lower rates of firearm-related homicides[5] and suicides,[12] fewer fatal mass shootings,[28] and fewer instances of law enforcement officers shot in the line of duty.[29]

Although there were increases in Missouri in both firearm and nonfirearm mortality, the differences in firearm mortality were 2.6 times larger. The increase in nonfirearm homicides coincident with the repeal of Missouri's licensing law may indicate that other factors affected mortality rates in Missouri after the repeal of its licensing law and that the actual effect on firearm mortality was somewhat smaller than our estimate. In a recent study incorporating data through 2016, there was an estimated 27% increase in firearm homicides when changes in Missouri were compared with those in states from the region with similarly high baseline homicide rates.[14]

Maryland's CBC law was associated with increases in homicide rates; however, the increases were specific to Baltimore and were

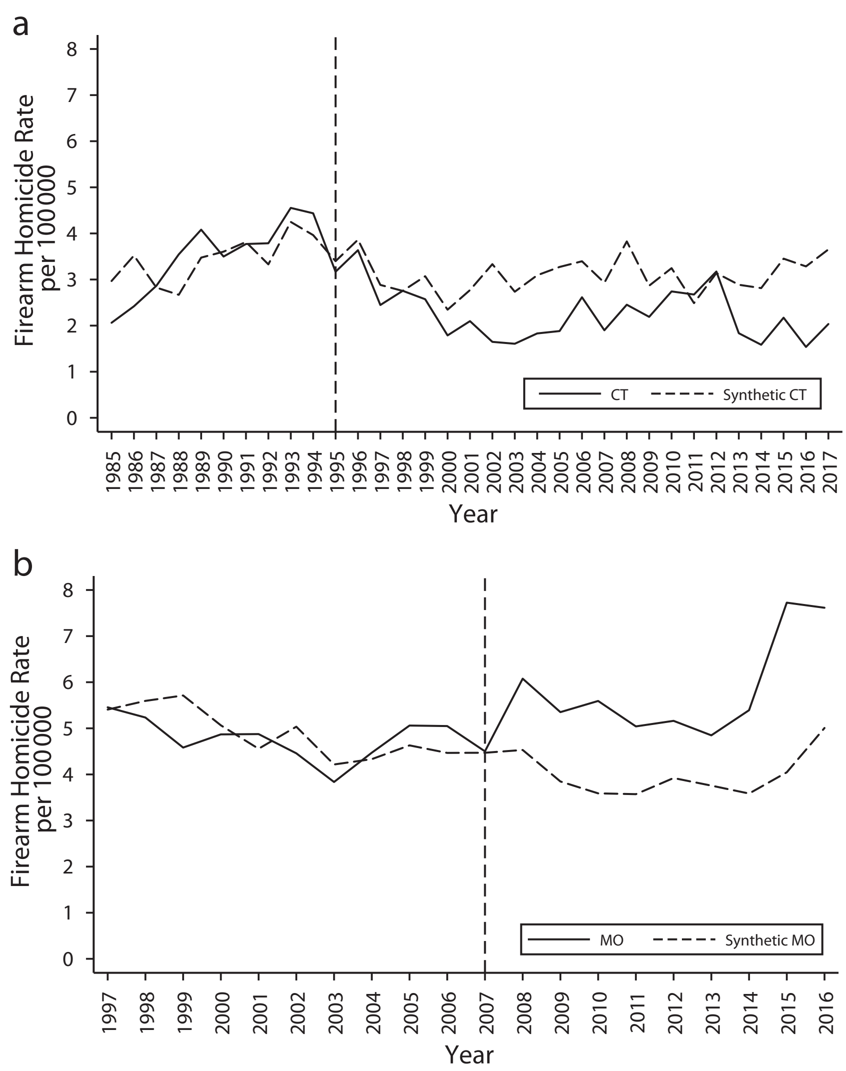

FIGURE 1—Effects of Purchaser Licensing Laws on Firearm Homicides in (a) Connecticut (Adopted 1995) and (b) Missouri (Repealed 2007)

not evident in the rest of the state. This suggests that either conditions in Baltimore modified the law's effect or the estimate of the law's effect was biased by unmeasured confounders. It is unclear how to interpret the positive association between Pennsylvania's CBC law and homicide rates. If the law substantially limited the ability of potential homicide victims to access firearms and successfully defend themselves, one would expect an even greater harmful effect of licensing. Yet, licensing laws were linked to lower homicide rates.

Consistent with previous longitudinal studies,[6,7] CBC laws in Maryland and Pennsylvania did not appear to reduce firearm suicides. Although Maryland experienced a decrease in firearm suicides after implementation of a CBC law, there was an even larger percentage decrease in nonfirearm suicides. This latter drop was more unusual in contrast to placebo states, suggesting that other factors may have been contributing to changing suicide rates in Maryland.

Comprehensive background check requirements may be necessary to prevent prohibited individuals from accessing firearms; without purchaser licensing requirements, however, they may be insufficient to achieve this objective and prevent lethal gun violence. The effectiveness of CBC laws could be enhanced by more robust efforts to enforce the laws and promote compliance, broader prohibiting conditions, better record keeping, and expanded time to complete the checks.[30] A prior study documented infrequent enforcement of Maryland's and Pennsylvania's CBC laws,[31] which weakens the laws' capacity to deter illegal transfers of firearms. When Maryland added handgun purchaser licensing requirements to its CBC law in 2013, diversions of guns for criminal use shortly after retail sales dropped dramatically.[32] This suggests that point-of-sale CBC requirements in Maryland were an insufficient deterrent to illegal diversions without purchaser licensing.

There are multiple reasons that firearm purchaser licensing might be more effective than point-of-sale CBC laws without licensing. Purchaser licensing requires vetting procedures that are more robust than is the case for point-of-sale CBC laws. This may deter individuals who might otherwise buy guns with the intention of criminal misuse or for transfer to a prohibited individual. States with purchaser licensing laws allow more time for vetting purchase applications and often check more complete sources of state data on prohibiting conditions than is the case under point-of-sale CBC laws. Firearm purchaser licensing also makes it easier for private sellers to verify that a prospective buyer is not prohibited from purchasing a firearm. Finally, licensing increases the real cost of purchasing firearms with additional time commitments and licensing fees. This likely reduces firearm ownership and the number of guns within a population.

The process required to obtain a purchaser license may also be protective with respect to suicide. It is much more difficult for individuals to make an impulsive decision to purchase a firearm if they need to secure a license first. Many suicide attempts occur within minutes or hours of initial suicidal ideation.[33] Longer waiting periods between applying to purchase firearms and receiving the firearms are associated with lower rates of firearm homicides and suicides.[34]

This study has potential limitations. First, we examined a limited number of law changes. For purchaser licensing, we assessed the only 2 law changes for which there were at least 5 years of postlaw data available. For CBC law changes, prior law changes since

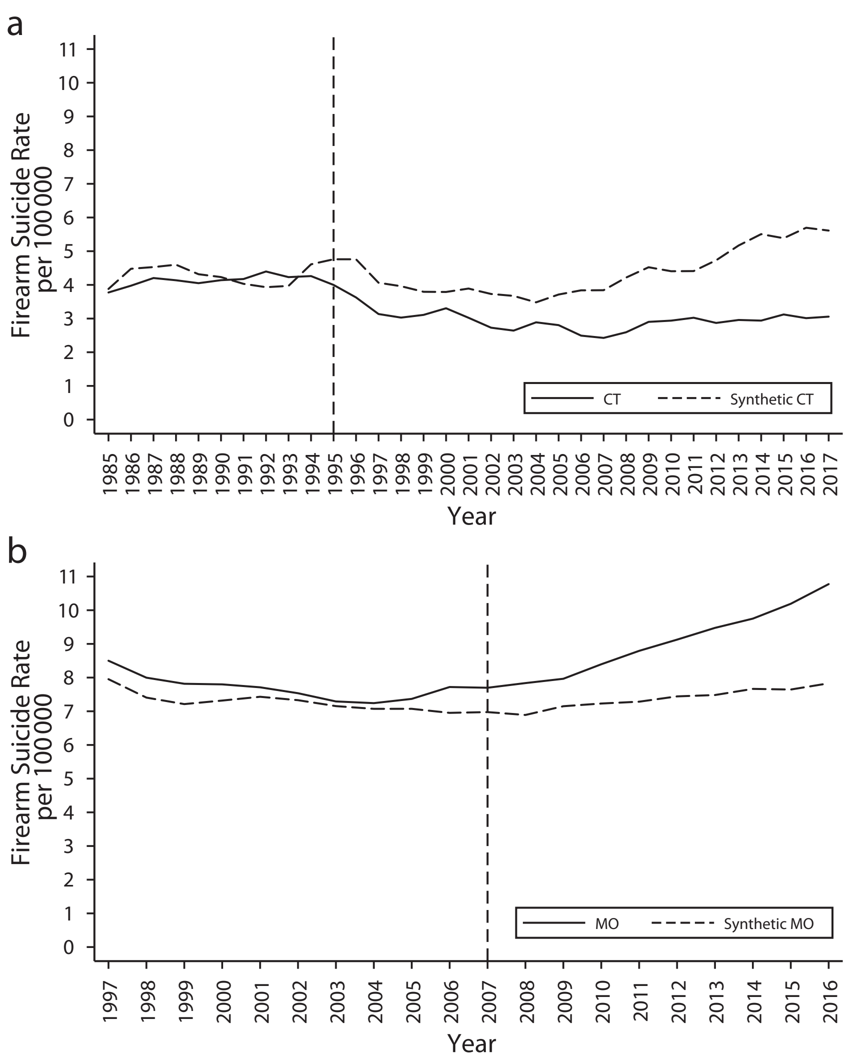

FIGURE 2—Effects of Purchaser Licensing Laws on Firearm Suicides in (a) Connecticut (Adopted 1995) and (b) Missouri (Repealed 2007)

1990 had already been evaluated,[9] and recent changes provided few postlaw data points. Second, although the synthetic control method is a robust strategy for estimating policy effects, the control pool for our analyses was somewhat limited in the case of Missouri.

Third, we sought to expand on previous work by extending the time period for each model to include the latest possible year of data. Our results are, therefore, more informative, but longer postlaw periods may create some uncertainty with respect to the capability of the models to accurately estimate the counterfactual. Finally, visual analysis of some of our synthetic control plots revealed that although the prelaw MSPE was minimized, there was a separation between the synthetic control and the treated state just before a law change. Such separation prior to a change could be random variation or could be indicative of unmeasured factors influencing trends between prelaw and postlaw change periods that might bias effect estimates.

Despite these limitations, our analyses have many strengths. We used a rigorous statistical method that minimizes errors in model prediction. We contrasted the patterns of estimated law effects across firearm and nonfirearm homicides and suicides to assess whether estimated effects were specific to deaths involving firearms. The CBC laws and one of the purchaser licensing laws were all adopted in 1995 or 1996, allowing for comparisons within the same historical period. We offered a fourth law change, Missouri's repeal of purchaser licensing during a time of relatively stability in homicide trends in Missouri and nationwide, to contrast with Connecticut's implementation of purchaser licensing in a different region and time period.

Although data on public support for firearm policies reveal somewhat broader support for CBC laws than is the case for purchaser licensing, a 2019 national survey reported 77% support for handgun purchaser licensing.[35] CBC laws are critical for keeping firearms from high-risk individuals, but they may be insufficient to significantly reduce firearm mortality without purchaser licensing. AJPH

CONTRIBUTORS

A. D. McCourt led the writing and analyses. A. D. McCourt, C. K. Crifasi, E. A. Stuart, J. S. Vernick, and D. W. Webster designed the study and statistical analyses. R. M. C. Kagawa and G. J. Wintemute provided critical review and interpretation of the data, analyses, and findings. All of the authors contributed to data interpretation and critical revisions of the article.

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CONFLICTS OF INTEREST

The authors have no conflicts of interest to disclose.

HUMAN PARTICIPANT PROTECTION

No protocol approval was needed for this study because secondary data sources were used to analyze aggregated mortality rates.

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